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Publiſhed the Laſt Day of every Month,

[PRICE TWO SHILLINGS.]

THE
PHILOSOPHICAL MAGAZINE.

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Engd by Mackenzie from an Original Picture.

Jos de Beauchamp

Astronomer.

Published by A. Tilloch Carey Street June 1st 1802.

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MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, ETC. ETC.

> "Nec aranearum ſane textus ideo melior, quia ex ſe fila gignunt. Nec noſter vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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I. *Reſearches reſpecting the Compoſition of Enamel.* By C. CLOUET, *Aſſociate of the French National Inſtitute**.

WHITE ENAMEL.

WHITE enamel, either for earthen-ware, or the purpoſe of being applied on metals, is compoſed in the following manner: You firſt calcine a mixture of lead and tin, which may be varied in the following proportions; *viz.* for 100 parts of lead, 15, 20, 30, and even 40 of tin. A mixture of lead and tin calcines very eaſily in contact with the air. As ſoon as this mixture is brought to a red heat, nearly a cherry colour, it burns like charcoal, and is calcined very ſpeedily. The compoſition which calcines beſt, is that which in 100 pounds of lead contains from 20 to 25 of tin. The tin here meant is pure tin. In proportion as the calcination is effected, you muſt take out the calcined part, and continue to oxydate the reſt until the whole has become pulverulent. As ſome ſmall particles always eſcape calcination, you muſt expoſe to the fire a ſecond time the oxyd obtained in order to calcine it completely; which may be eaſily known by its ceaſing to ſparkle; that is to ſay, when you no longer ſee any parts burn like coal, and when the whole appears of an uniform colour. When the proportion of tin exceeds 25 or 30, a ſtronger fire is neceſſary to produce the calcination. In a word, by varying the degrees of heat you will be able to diſcover that beſt ſuited to the mixture on which you operate.

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* From the *Annales de Chimie.*

A hundred parts of the calx above mentioned, which in the French potteries is called *calcine*, is generally taken with 100 parts of ſand. From 25 to 30 pounds of ſea-ſalt, or muriat of ſoda, are added: the whole is well mixed together, and it is fuſed in the bottom of a furnace in which potter's ware is baked. This matter is generally placed on ſand, on lime quenched in the open air, or on aſhes. The bottom of the maſs is in general badly fuſed. This, however, does not prevent the matter, after it has been pounded, and applied on the articles, from becoming exceedingly white and hard in the furnace. When taken from the furnace it is not white; it is even often very black: in general it is marbled with black, gray, and white.

This proceſs is that generally uſed in potteries. In the compoſitions deſtined for earthen-ware, the proportion of 25 parts of tin to 100 of lead is never exceeded: for common earthen-ware, the manufacturers are even ſatisfied with 15 of tin to 100 of lead. It may be eaſily ſeen, that if you wiſh to obtain an enamel whiter and more fuſible, you muſt diminiſh the quantity of ſand; but there is no neceſſity for augmenting that of the ſea-ſalt, or muriat of ſoda: as the whiteneſs and opacity depend on the quantity of tin, you may uſe *calcine*, which contains 25 or 30 *per cent.* For example, 100 of ſuch calcine, 60 of ſand, and 25 of marine ſalt, give a compoſition exceedingly fuſible.

But it is to be obſerved, that it is neceſſary to employ ſome further manipulations when you wiſh to have enamel proper for being applied on metal, and are deſirous to give it all the perfection of which it is ſuſceptible. In that caſe, you do not employ crude ſand, but calcine it, in a ſtrong heat, with a quarter of its weight of marine ſalt, either in a ſmall quantity in a crucible, or on a large ſcale in a potter's furnace. If you wiſh to have a very fuſible enamel, you may even add minium, or lead calcined by the former operation, and nearly as much ſea-ſalt, that is to ſay, a fourth. You then obtain a white maſs half fuſed and porous, which you pulveriſe, and employ in the compoſition of enamel inſtead of ſand, and in the ſame proportions as ſand: you may even diminiſh the quantity of this matter to 50 *per cent.* if you are deſirous to obtain an enamel very fuſible. This will depend alſo on the *calcine* employed; for that which is moſt charged with tin is the leaſt fuſible.

When you wiſh to have fluxes for the colours, you employ the ſame compoſitions before mentioned, except that you put little or no tin into the lead. In the latter caſe you muſt generally employ minium. This flux is good for certain colours, but not for all. There are ſome which become tarniſhed by fluxes, that contain the oxyds of lead. In that caſe, you muſt make fluxes without oxyd of lead. Nitre and borax are generally uſed for making this glaſs, but you add no calx of tin. The following are thoſe which I have tried:

Three parts of ſiliceous ſand, one of chalk, and three of calcined borax, give a matter proper to be uſed as a flux for purples, blues, and other delicate colours.

Three parts of white or flint glaſs, one of calcined borax, a quarter of a part of nitre, one of the white oxyd of antimony made with nitre well waſhed, give an exceedingly white enamel, which may ſerve alſo as a flux for purple, and particularly for blue.

Sixty parts of enamel ſand or leſs, thirty of alum, thirty-five of ſea-ſalt, and a hundred of minium, or any other oxyd of lead, give a white enamel when the fluxes do not predominate too much, and a gelatinous glaſs when a great deal of fluxes has been added. This glaſs is good for red, and the enamel may be applied to all kinds of clay capable of ſuſtaining a ſtrong heat.

It is of great importance to remark, and to know, that the ſand employed for enamel muſt not be ſand which contains only ſilex: ſand of that kind alone is of no uſe. The ſand proper for this purpoſe is that which contains talc with ſilex. To make a ſand proper for enamel and the fluxes of colours, &c. there muſt be nearly one part of talc and three of ſiliceous ſand.

What appears to me moſt eſſential in regard to the ſucceſs of enamel, is the choice of ſand. It is very poſſible to compoſe this ſand by art; and though I have not decompoſed it, I have found by ſyntheſis, that three parts of ſiliceous ſand and one of talc form an excellent ſand for enamel. From this it may be readily ſeen, that, to compoſe with facility ſand for enamel, nothing is neceſſary but to determine, by a good analyſis, the quantity of talc. This ſand may be procured in places where earthen-ware is made. It may be eaſily known; for, beſides the ſiliceous ſand, which forms the greateſt part of it, you may obſerve in it talcky particles in great abundance; and, to be good, it muſt contain nearly a quarter. When it does not contain a ſufficient quantity, the enamel it produces fuſes with more difficulty, and does not become ſmooth; it remains granulated and pitted. There are certainly ſome combinations of earth which may produce very good fluxes, either for enamel or for tranſparent colours. It might be attended with advantage to try ſome of theſe combinations. Ponderous earth (barytes) and lime fuſe very well together: by adding a little ſilex, or a little magneſia, it is probable that an excellent matter might be produced. If this glaſs, compoſed of lime and barytes only, had ſufficient ſolidity to reſiſt the air and weak acids, there would be no neceſſity perhaps to add ſilex; but if the marine ſalt, as I am inclined to think, ought alſo to enter into the compoſition of this kind of glaſs, ſilex ought likewiſe to form a part of it. The experiments on this head, for the ſake of trial, may be varied different ways. When the glaſs deſtined to ſerve as flux for colours is employed, it is cuſtomary, in order that they may be rendered more fuſible, to add a little nitre and borax. The common borax of the ſhops contains an exceſs of ſoda, which, in my opinion, it would be of benefit to ſaturate with the nitric acid. I think alſo that the flux might be rebaked with the doſe of nitre and borax, or of nitric borax, which might be added before being employed. It is only to colours ſuch as purple and the oxyd of cobalt that nitre and borax are added.

I have tried to find a ſubſtitute for marine ſalt in the compoſition of white enamel. Potaſh produced only an ugly and ill fuſed gray maſs, which acquired no luſtre in the furnace; nitre produced a green maſs, but exceedingly friable;

ſulphat of potaſh produced very nearly the ſame effect, only the maſs was a little whiter: but neither of theſe enamels was worth any thing. I did not try pure ſoda: I have, however, heard common ſoda extolled; but as it contains a great deal of marine ſalt, it muſt undoubtedly be on account of this ſalt that it produces a good effect. Pure ſoda may nevertheleſs be tried, either alone or with marine ſalt; it perhaps might produce no bad effect with potaſh.

I have tried alſo a mixture of equal parts of lime and argil, to which I added one part of ſilex, and likewiſe without ſilex; but this mixture did not ſupply the place of talcky ſand. This ſand is not in general found in grains; it exhibits itſelf moſt commonly under the form of a ſtone, ſuch as free-ſtone; but ſome of it is found alſo in grains.

We ſhould be much deceived in making white enamel were we to employ the oxyds of tin and lead ſeparately, as I have read in all the authors I could find who treat on the art of pottery. None of them ſay what they ought reſpecting enamel, nor even reſpecting the compoſition or nature of the earth proper for bearing an enamel.

It is eſſential that the lead and tin for making the oxyd deſtined to produce white enamel, ſhould be fuſed and mixed together before they are calcined; and if you wiſh that the enamel ſhould immediately acquire its full whiteneſs, it will be requiſite that the calcination ſhould be complete.

Biſmuth might perhaps be employed as a ſubſtitute for the lead, and it is not improbable that it would give a good product. Biſmuth alſo might be mixed with the lead in the following manner; *viz.* one part of lead, one of biſmuth, and one of tin: or other proportions might be employed; but I have not tried any others. As the oxyd of biſmuth, however, is exceedingly fuſible, I think it might be admitted, with great advantage, into certain fluxes. I have not tried what might be produced by the white calx of zinc, nor by that of tin, made by diſſolving it in the nitric acid or by detonation with nitre. A mixture of lead and tin, detonated with nitre, would be uſeful. Though the white calx of regulus of antimony made by nitre, and well waſhed, (diaphoretic antimony,) produces a very beautiful white enamel when fuſed with three parts of white glaſs, (which contains neither lead nor other metallic oxyds,) and one of glaſs of borax, with a half or fourth part of nitre; yet this calx, ſo white when mixed with the compoſition of enamel, made with enamel ſand and the combined oxyd of lead and tin, inſtead of increaſing, tarniſhes the whiteneſs, and only gives a blueiſh enamel of a livid colour *. Perhaps enamels, completely made and mixed together in the firſt inſtance, would not produce the ſame effect; but this I never tried. I have, however, employed this compoſition as a flux for colours, which, applied afterwards on the enamel of earthen-ware, preſerved its beauty. I put ſome of this pure enamel alſo over that of earthen-ware, and I think it preſerved its whiteneſs.

The principal quality of good enamel, and that which renders it fit for being applied on baked earthen-ware, or on metals, is the facility with which it acquires luſtre by a moderate heat, (a cherry-red heat, more or leſs, according to the nature of the enamel,) without entering into complete fuſion †. Enamels applied to earthen-ware and metals poſſeſs this quality. They do not enter into complete fuſion; they aſſume only the ſtate of paſte, but of a paſte exceedingly firm; and yet when baked one might ſay that they had been completely fuſed.

There are two methods of painting on enamel: on raw or on baked enamel. Both theſe methods are employed, or may be employed, for the ſame object. Solid colours, capable of ſuſtaining the fire neceſſary for baking the enamel ground, may be applied in the form of fuſed enamel on that which is raw, and the artiſt may afterwards finiſh with the tender colours. The colours applied on the raw material do not require any flux; there is one, even, to which ſilex muſt be added, that is, the calx of copper, which gives a very

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* Antimony, employed in any manner as a glazing for earthen-ware, would be more dangerous than lead; even the latter ſhould, if poſſible, be diſcarded.—Edit.

† The ingenious author has omitted another principal quality. It ought never to contain ſuch a portion of deliqueſcing ſalts, as to endanger its being afterwards injured by water. This takes place oftener than is generally ſuſpected.—Edit.

beautiful green: but when you wiſh to employ it on the raw material, you muſt mix with it about two parts of its weight of ſilex, and bring the mixture into combination by means of heat. You afterwards pulveriſe the maſs you have thus obtained, in order to employ it.

To obtain good white enamel, it is of great importance that the lead and tin ſhould be very pure. If theſe metals contain copper or antimony, as is often the caſe, the enamel will not be beautiful: iron is the leaſt hurtful.

OF COLOURED ENAMELS.

All the colours may be produced by the metallic oxyds. Theſe colours are more or leſs fuſed in the fire, according as they adhere with more or leſs ſtrength to their oxygen. All metals which readily loſe their oxygen cannot endure a great degree of heat, and are unfit for being employed on the raw material.

Purple.

This colour is the oxyd of gold, which may be prepared different ways; as by precipitating, by means of a muriatic ſolution of tin, a nitro-muriatic ſolution of gold much diluted in water. The leaſt quantity poſſible of the ſolution of tin will be ſufficient to form this precipitate. The ſolution of tin muſt be added gradually until you obſerve the purple colour begin to appear: you then ſtop; and having ſuffered the colour to be depoſited, you put it into an earthen veſſel to dry ſlowly*. The different ſolutions of gold, in whatever manner precipitated, provided the gold is precipitated in the ſtate of an oxyd, give always a purple colour, which will be more beautiful in proportion to the purity of the oxyd; but neither the copper nor ſilver, with which gold is generally found allayed, injure this colour in a ſenſible manner: it is changed, however, by iron. The gold precipitate which gives the moſt beautiful purple is certainly fulminating gold, which loſes that property when mixed with fluxes. Purple is an abundant colour; it is capable of bearing a great deal of flux, and in a ſmall quantity communicates its colour to a great deal of matter. It appears that ſaline fluxes are better ſuited to it than thoſe in which there are metallic calces. Thoſe, therefore, which have been made with ſilex, chalk, and borax, or white glaſs, borax, and a little white oxyd of antimony, with a little nitre, as I have already mentioned, ought to be employed with it. Purple will bear from four to twenty parts of flux, and even more, according to the ſhade required. Painters in enamel employ generally for purple a flux which they call brilliant white. This flux appears to be a ſemi-opake enamel, which has been drawn into tubes, and afterwards blown into a ball at an enameller's lamp. Theſe bulbs are afterwards broken in ſuch a manner that the flux is found in ſmall ſcales, which appear like the fragments of ſmall hollow ſpheres. Enamel painters mix this flux with a little nitre and borax. This matter, which produces a very good effect, I employed, without attempting to decompoſe it. It may be a very fuſible common white enamel which has been blown into that form. It is to be remarked, that purple will not bear a ſtrong heat.

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* The colour is always more beautiful, if the precipitate is ground with the flux before it has become dry.—Edit.

Red.

We have no metallic oxyd capable of giving directly a fuſed red; that is to ſay, we have no metallic calces which, entering into fuſion, and combining under the form of tranſparent glaſs with fluxes or glaſs, give directly a red colour. To obtain this colour it muſt be compounded different ways, as follows:—Take two parts, or two parts and a half (you may, however, take only one part,) of ſulphat of iron and of ſulphat of alumine; fuſe them together in their water of cryſtalliſation, and take care to mix them well together. Continue to heat them to complete dryneſs; then increaſe the fire ſo as to bring the mixture to a red heat. The laſt operation muſt be performed in a reverberating furnace. Keep the mixture red until it has every where aſſumed a beautiful red colour, which you may aſcertain by taking out a little of it from time to time, and ſuffering it to cool in the air. You may then ſee whether the matter is ſufficiently red: to judge of this it muſt be left to cool, becauſe while hot it appears black. The red oxyds of iron give a red colour; but this colour is exceedingly fugitive; for, as ſoon as the oxyd of iron enters into fuſion, the portion of oxygen which gives it its red colour leaves it, and it becomes black, yellow, or greeniſh. To preſerve, therefore, the red colour of this oxyd in the fire, it muſt be prevented from vitrifying, and abandoning its oxygen; which may be accompliſhed by the method I have indicated. I have tried a variety of different ſubſtances to give it this fixity, but none of them ſucceeded except alum. The doſes of alum and ſulphat of iron may be varied. The more alum you add, the paler will be the colour. Three parts of alum to one of ſulphat of iron give a colour which approaches fleſh colour. It is alum alſo which gives this colour the property of becoming fixed at a very ſtrong heat. This colour may be employed on raw enamel: it has much more fixity than the purple, but not ſo much as the blue of cobalt. It may be waſhed to carry off the ſuperfluous ſaline matter, but it may be employed alſo without edulcoration; in that ſtate it is even more fixed and more beautiful. It does not require much flux; the flux which appeared to me to be beſt ſuited to it, is compoſed of alum, minium, marine ſalt, and enamel ſand. This flux muſt be compounded in ſuch a manner as to render it ſufficiently fuſible for its object: from two to three parts of it are mixed with the colour. In general, three parts of flux are uſed for one of colour: but this doſe may, and ought to be varied according to the nature of the colour and the ſhade of it required. Red calx of iron alone, when it enters into fuſion with glaſs, gives a colour which ſeems to be black; but if the colour be diluted with a ſufficient quantity of glaſs, it at laſt becomes of a tranſparent yellow. Thus, the colour really produced by calx of iron combined with glaſs is a yellow colour, but which being accumulated becomes ſo dark that it appears black. In the proceſs above given for making the red colour, the oxyd of iron does not fuſe: and this is the eſſential point; for, if this colour is carried in the fire to vitrification, it becomes black, or yellowiſh, and diſappears if the coat be thin, and the oxyd of iron preſent be only in a ſmall quantity.

Yellow.

Though yellow may be obtained in a direct manner, compound yellows are preferred; becauſe they are more certain in their effect, and more eaſily applied than the yellow,

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which may be directly obtained from ſilver. The compound yellows are obtained in conſequence of the ſame principles as the red colour of iron. For this purpoſe we employ metallic oxyds, the vitrification of which muſt be prevented by mixing with them other ſubſtances, ſuch as refractory earths, or metallic oxyds difficult to be fuſed.

The metallic calces which form the baſes of the yellow colours are generally thoſe of lead; as minium, the white calx of lead, or litharge, the white calx of antimony, called diaphoretic antimony: that called crocus metallorum is alſo employed. This regulus, pulveriſed and mixed with white oxyd, gives likewiſe a yellow. The following are the different compoſitions uſed: one part of the white oxyd of antimony, one of the white oxyd of lead (or two or three); theſe doſes are exceedingly variable; one part of alum, and one of ſal-ammoniac. When theſe matters have been all pulveriſed, and mixed well together, they are put in a veſſel over a fire ſufficient to ſublimate and decompoſe the ſal-ammoniac; and when the matter has aſſumed a yellow colour, the operation is finiſhed *. The calces of lead mixed in a ſmall quantity either with ſilex or alumine, alſo with the pure calx of tin, exceedingly white, give likewiſe yellows. One part of the oxyd of lead is added to two, three, or four of the other ſubſtances above mentioned. In theſe different compoſitions for yellow, you may uſe alſo oxyd of iron, either pure, or that kind which has been prepared with alum and vitriol of iron: you will then obtain different ſhades of yellow. From what has been ſaid, you may vary theſe compoſitions of yellow as much as you pleaſe. Yellows require ſo little flux, that one or two parts, in general, to one of the colour are ſufficient; ſaline fluxes are improper for them, and eſpecially thoſe which contain nitre. They muſt be uſed with fluxes compoſed of enamel ſand, oxyd of lead, and borax, without marine ſalt.

A yellow may be obtained alſo directly from ſilver. All theſe mixtures may be varied, and you may try others. For this purpoſe you may uſe ſulphat of ſilver, or any oxyd of that metal mixed with alumine or ſilex, or even with both,

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* This in colour-ſhops is called Naples yellow.

in equal quantities. The whole muſt be gently heated until the yellow colour appear, and the matter is to be employed with the fluxes pointed out for yellows. Yellow of ſilver, like purple, cannot endure a ſtrong heat: a nitric ſolution of ſilver may be precipitated by the ammoniacal phoſphat of ſoda, and you will obtain a yellow precipitate, which may be uſed to paint in that colour with fluxes, which ought then to be a little harder.

Beſides the methods above mentioned, the beſt manner of employing the oxyd of ſilver is, in my opinion, to employ it pure: in that caſe, you do not paint, but ſtain. It will be ſufficient, then, to lay a light coating on the place which you wiſh to ſtain yellow, and to heat the article gently to give it the colour. You muſt not employ too ſtrong a heat: the degree will eaſily be found by practice. When the article has been ſufficiently heated, you take it from the fire and ſeparate the coating of oxyd, which will be found reduced to a regulus: you will then obſerve the place which it occupied tinged of a beautiful yellow colour, without thickneſs. It is chiefly on tranſparent glaſs that this proceſs ſucceeds beſt. Very fine ſilver filings produce the ſame effect: but what ſeemed to me to ſucceed beſt in this caſe, was ſulphat of ſilver, well ground up with a little water, that it may be extended very ſmooth. From what has been ſaid, it may readily be ſeen that this yellow muſt not be employed like other colours; that it muſt not be applied till the reſt have been fuſed; for, as it is exceedingly fuſible, and ready to change, it would be injured by the other colours; and as the coating of ſilver which is reduced muſt be removed, the fluxes would fix it, and prevent the poſſibility of its being afterwards ſeparated. Working on glaſs is not attended with this inconvenience, becauſe the ſilver-yellow is applied on the oppoſite ſide to that on which the other colours are laid.

Green.

Green is obtained directly from the oxyd of copper. All the oxyds of copper are good: they require little flux, which even muſt not be too fuſible: one part or two of flux will be ſufficient for one oxyd. This colour agrees with all the fluxes, the ſaline as well as the metallic; which tends to vary a little the ſhades. I have already pointed out the method of employing theſe oxyds on raw enamel: were not that method only followed in this caſe, the oxyd of copper would extend, and ſpread itſelf, like a cloud, beyond the limits preſcribed for it.

A mixture of yellow and blue is alſo uſed to produce green. Thoſe who paint figures or portraits employ glaſs compoſed in this manner; but thoſe who paint glazed veſſels, either earthen-ware or porcelain, employ in general copper green.

Independently of the beautiful green colour produced by oxydated copper, it produces alſo a very beautiful red colour; but I do not know that it is employed on enamel. This beautiful red colour, produced by copper, is exceedingly fugitive. The oxyd of copper gives red only when it contains very little oxygen, and approaches near to the ſtate of a regulus. Notwithſtanding the difficulty of employing this oxyd for a red colour, a method has been found to ſtain tranſparent glaſs with different ſhades of a very beautiful red colour by means of calx of copper. The proceſs is as follows: You do not employ the calx of copper pure, but add to it calx of iron, which for that purpoſe muſt not be too much calcined; you add alſo a very ſmall quantity of calx of copper to the maſs of glaſs which you are deſirous of tingeing. This glaſs at firſt muſt have only a very ſlight tinge of green, inclining to yellow. When the glaſs has that colour you make it paſs to red, and even a very dark red, by mixing with it red tartar in powder, and even tallow. You muſt mix this matter well in the glaſs, and it will aſſume a very dark red colour. The glaſs ſwells up very much by this addition. Before it is worked it muſt be ſuffered to ſettle, and become compact; but as ſoon as it has fully aſſumed the colour it muſt be immediately worked, for the colour does not remain long, and even often diſappears while working; but it may be reſtored by heating the glaſs at the flame of a lamp. It is exceedingly difficult to make this colour well; but when it ſucceeds it is very beautiful, and has a great deal of ſplendour. By employing the calx of copper alone for the proceſſes above mentioned you will obtain, when you ſucceed well, a red ſimilar to the moſt beautiful carmine. The calx of iron changes the red into vermilion, according to the quantity added. If we had certain proceſſes for making this colour, we ſhould obtain all the ſhades of red from pure red to orange, by uſing, in different proportions, the oxyd of copper and that of iron. The calx of copper fuſes argil more eaſily than ſilex: the caſe is the ſame with calx of iron. If you fuſe two or three parts of argil with one of the oxyd of copper, and if the heat be ſufficient, you will obtain a very opake enamel, and of a vermilion red colour: the oxyd of copper paſſes from red to green through yellow; ſo that the enamel of copper, which becomes red at a ſtrong heat, may be yellow with a weaker heat. The ſame effect may be produced by deoxydating copper in different degrees: this will be effected according as the heat is more or leſs violent. The above compoſition might, I think, be employed to give a vermilion red colour to porcelain. The heat of the porcelain furnace ought to be of ſufficient ſtrength to produce the proper effect.

The calx of iron fuſed alſo with argil in the ſame proportions as the calx of copper, gives a very beautiful black. Theſe proportions may, however, be varied.

Blue.

Blue is obtained from the oxyd of cobalt. It is the moſt fixed of all colours, and becomes equally beautiful with a weak as with a ſtrong heat. The blue produced by cobalt is more beautiful the purer it is, and the more it is oxydated. Arſenic does not hurt it. The ſaline fluxes which contain nitre are thoſe beſt ſuited to it: you add a little alſo when you employ that flux which contains a little calcined borax or glaſs of borax, though you may employ it alſo with that flux alone.

But the flux which, according to my experiments, gives to cobalt blue the greateſt ſplendour and beauty, is that compoſed of white glaſs (which contains no metallic calx), of borax, nitre, and diaphoretic antimony well waſhed. When this glaſs is made for the purpoſe of being employed as a flux for blue, you may add leſs of the white oxyd of antimony: a ſixth of the whole will be ſufficient.

Violet.

Black calx of manganeſe, employed with ſaline fluxes, gives a very beautiful violet. By varying the fluxes, the ſhade of the colour may alſo be varied: it is very fixed as long as it retains its oxygen. The oxyd of manganeſe may produce different colours; but for that purpoſe it will be neceſſary that we ſhould be able to fix its oxygen in it in different proportions. How to effect this, as far as I know, has never yet been diſcovered.

Theſe are all the colours obtained from metals. From this it is evident that ſomething ſtill remains to be diſcovered. We do not know what might be produced by the oxyds of platina, tungſten, molybdena, and nickel: all theſe oxyds are ſtill to be tried; each of them muſt produce a colour, and perhaps red, which is obtained neither directly nor with facility from any of the metallic ſubſtances formerly known and hitherto employed.

GENERAL REMARKS.

Thoſe who paint on enamel, on earthen-ware, porcelain, &c. muſt regulate the fuſibility of the colours by the moſt tender of thoſe employed, as for example, the purple. When the degree which is beſt ſuited to purple has been found, the other leſs fuſible colours may be ſo regulated, (by additions of flux,) when it is neceſſary to fuſe all the colours at the ſame time, and at the ſame degree of heat.

You may paint alſo in enamel without flux; but all the colours do not equally ſtand the heat which muſt be employed. If the enamel, however, on which you paint be very fuſible, they may all penetrate it. This manner of painting gives no thickneſs of colour; on the contrary, the colours ſink into the enamel at the places where the tints are ſtrongeſt. To make them penetrate, and give them luſtre, a pretty ſtrong fire will be neceſſary to ſoften the enamel and bring it to a ſtate of fuſion. This method cannot be practiſed but on enamel compoſed with ſand, which I call enamel ſand, as already mentioned. It may be readily ſeen, alſo, that the colours and enamel capable of enduring

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the greateſt heat, will be the moſt ſolid, and the leaſt liable to be changed by the air. An account of the method of employing and baking enamel may be found in various works, and may be learned alſo by ſeeing the operations of enamellers.

II. *On a new fulminating Mercury.* *By* EDWARD HOWARD, *Eſq. F.R.S.*[*]

SECTION I.

THE mercurial preparations which fulminate, when mixed with ſulphur, and gradually expoſed to a gentle heat, are well known to chemiſts: they were diſcovered, and have been fully deſcribed, by Mr. Bayen[†].

MM. Brugnatelli and Van Mons have likewiſe produced fulminations by concuſſion, as well with nitrat of mercury and phoſphorus, as with phoſphorus and moſt other nitrats[‡]. Cinnabar, likewiſe, is amongſt the ſubſtances which, according to MM. Fourcroy and Vauquelin, detonate by concuſſion with oxymuriat of potaſh[§].

Mr. Ameilon had, according to Mr. Berthollet, obſerved, that the precipitate obtained from nitrat of mercury by oxalic acid fuſes with a hiſſing noiſe[‖].

SECTION

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[*] From *Tranſactions of the Royal Society of London for* 1800.

[†] *Opuſcules Chimiques de Bayen*, Tom. I. p. 346, and note in p. 344.

[‡] *Annales de Chimie*, Tom. XXVII. p. 74 and 79.

[§] *Ibid.* Tom. XXI. p. 238.

[‖] This fact has been miſrepreſented in the introduction to a work entitled The Chemical Principles of the Metallic Arts, by W. Richardſon, Surgeon, F. A. S. Sc. (page lvii.) The author, ſpeaking of the acid of ſorrel, ſays, " Klaproth of Berlin precipitated a nitrous ſolution of mercury with acid of wood-ſorrel neutraliſed with vegetable alkali. The white precipitate, well waſhed and dried, produced a fulminating noiſe not inferior to that of fulminating gold. Acid of ſugar, perfectly neutraliſed by vegetable alkali, produced the ſame precipitate, which, on expoſure to heat, exhibited the ſame fulminating power." I muſt confeſs, I have not been able to produce any ſuch fulmination. Mr. Richardſon has moreover given this ſuppoſed diſcovery to Mr. Klaproth; whereas Mr. Berthollet, when quoting the fact to which I ſuppoſe Mr. Richardſon intended to allude, obſerves, " Qu'on avoit déjà donné le nom d'argent

SECTION II.

But mercury, and moſt, if not all its oxyds, may, by treatment with nitric acid and alcohol, be converted into a whitiſh cryſtalliſed powder, poſſeſſing all the inflammable properties of gunpowder, as well as many peculiar to itſelf.

I was led to this diſcovery by a late aſſertion, that hydrogen is the baſis of the muriatic acid: it induced me to attempt to combine different ſubſtances with hydrogen and oxygen. With this view, I mixed ſuch ſubſtances with alcohol and nitric acid, as I thought might (by prediſpoſing affinity) favour, as well as attract, an acid combination, of the hydrogen of the one, and the oxygen of the other. The pure red oxyd of mercury appeared not unfit for this purpoſe; it was therefore intermixed with alcohol, and upon both nitric acid was affuſed. The acid did not act upon the alcohol ſo immediately as when theſe fluids are alone mixed together, but firſt gradually diſſolved the oxyd: however, after ſome minutes had elapſed, a ſmell of ether was perceptible, and a white denſe ſmoke, much reſembling that from the *liquor fumans* of Libavius, was emitted with ebullition. The mixture then threw down a dark-coloured precipitate, which by degrees became nearly white. This precipitate I ſeparated by filtration; and, obſerving it to be cryſtalliſed in ſmall acicular cryſtals, of a ſaline taſte, and alſo finding a part of the mercury volatiliſed in the white fumes, I muſt acknowledge I was not altogether without hopes that muriatic acid had been formed, and united to the mercurial oxyd. I therefore, for obvious reaſons, poured ſulphuric acid upon the dried cryſtalline maſs; when a violent effervеſcence enſued, and, to my great aſtoniſhment, an exploſion took place.

The ſingularity of this exploſion induced me to repeat the

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fulminant au précipité du nitrate d'argent par l'acide oxalique, dans lequel M. Klaproth avoit découvert la propriété de fuſer avec vivacité lorſqu'on l'expoſe à la chaleur. M. Ameilon avoit auſſi, depuis longtems, fait connoitre que l'acide oxalique communiquoit cette propriété au mercure, quoique moins fortement qu'à l'argent; mais cet effet (he continues) eſt fort éloigné de celui qu'on déſigne par la fulmination."—*Annales de Chimie*, Tom. I. p. 57.

proceſs ſeveral times; and, finding that I always obtained the ſame kind of powder, I prepared a quantity of it, and was led to make the ſeries of experiments which I ſhall have the honour to relate in this paper.

SECTION III.

I firſt attempted to make the mercurial powder fulminate by concuſſion; and for that purpoſe laid about a grain of it upon a cold anvil, and ſtruck it with a hammer, likewiſe cold: it detonated ſlightly, not being, as I ſuppoſe, ſtruck with a flat blow; for, upon uſing three or four grains, a very ſtunning diſagreeable noiſe was produced, and the faces both of the hammer and the anvil were much indented.

Half a grain, or a grain, if quite dry, is as much as ought to be uſed on ſuch an occaſion.

The ſhock of an electrical battery, ſent through five or ſix grains of the powder, produces a very ſimilar effect: it ſeems, indeed, that a ſtrong electrical ſhock generally acts on fulminating ſubſtances like the blow of a hammer. Meſſrs. Fourcroy and Vauquelin found this to be the caſe with all their mixtures of oxymuriat of potaſh *.

To aſcertain at what temperature the mercurial powder explodes, two or three grains of it were floated on oil, in a capſule of leaf tin; the bulb of a Fahrenheit's thermometer was made juſt to touch the ſurface of the oil, which was then gradually heated till the powder exploded, as the mercury of the thermometer reached the 368th degree.

SECTION IV.

Deſirous of comparing the ſtrength of the mercurial compound with that of gunpowder, I made the following experiment, in the preſence of my friend Mr. Abernethy.

Finding that the powder could be fired by flint and ſteel, without a diſagreeable noiſe, a common gunpowder proof, capable of containing eleven grains of fine gunpowder, was filled with it, and fired in the uſual way: the report was ſharp, but not loud. The perſon who held the inſtrument in his hand felt no recoil; but the exploſion laid open the

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* *Annales de Chimie*, Tom. XXI. p. 239.

upper part of the barrel nearly from the touch-hole to the muzzle, and ſtruck off the hand of the regiſter, the ſurface of which was evenly indented to the depth of 0,1 of an inch, as if it had received the impreſſion of a punch.

The inſtrument uſed in this experiment being familiarly known, it is therefore ſcarcely neceſſary to deſcribe it; ſuffice it to ſay, that it was of braſs, mounted with a ſpring regiſter, the moveable hand of which cloſed up the muzzle, to receive and graduate the violence of the exploſion. The barrel was half an inch in caliber, and nearly half an inch thick, except where a ſpring of the lock impaired half its thickneſs.

SECTION V.

A gun belonging to Mr. Keir, an ingenious artiſt of Camden-town, was next charged with 17 grains of the mercurial powder, and a leaden bullet. A block of wood was placed at about eight yards from the muzzle to receive the ball, and the gun was fired by a fuſe. No recoil ſeemed to have taken place; as the barrel was not moved from its poſition, although it was in no ways confined. The report was feeble: the bullet, Mr. Keir conceived, from the impreſſion made upon the wood, had been projected with about half the force it would have been by an ordinary charge, or 68 grains, of the beſt gunpowder. We therefore recharged the gun with 34 grains of the mercurial powder; and, as the great ſtrength of the piece removed any apprehenſion of danger, Mr. Keir fired it from his ſhoulder, aiming at the ſame block of wood. The report was like the firſt in Section IV, ſharp, but not louder than might have been expected from a charge of gunpowder. Fortunately, Mr. Keir was not hurt, but the gun was burſt in an extraordinary manner. The breech was what is called a patent one, of the beſt forged iron, conſiſting of a chamber 0,4 of an inch thick all round, and 0,4 of an inch in caliber; it was torn open and flawed in many directions, and the gold touch-hole driven out. The barrel, into which the breech was ſcrewed, was 0,5 of an inch thick; it was ſplit by a ſingle crack three inches long, but this did not appear to me to be the immediate effect of the exploſion. I think the ſcrew of the breech, being ſuddenly enlarged, acted as a wedge upon the barrel. The ball miſſed the block of wood and ſtruck againſt a wall, which had already been the receptacle of ſo many bullets, that we could not ſatisfy ourſelves about the impreſſion made by this laſt.

SECTION VI.

As it was pretty plain that no gun could confine a quantity of the mercurial powder ſufficient to project a bullet with a greater force than an ordinary charge of gunpowder, I determined to try its comparative ſtrength in another way.

I procured two blocks of wood, very nearly of the ſame ſize and ſtrength, and bored them with the ſame inſtrument to the ſame depth. The one was charged with half an ounce of the beſt Dartford gunpowder, and the other with half an ounce of the mercurial powder; both were alike buried in ſand, and fired by a train communicating with the powders by a ſmall touch-hole. The block containing the gunpowder was ſimply ſplit into three pieces; that charged with the mercurial powder was burſt in every direction, and the parts immediately contiguous to the powder were abſolutely pounded; yet the whole hung together, whereas the block ſplit by the gunpowder had its parts fairly ſeparated. The ſand ſurrounding the gunpowder was undoubtedly moſt diſturbed: in ſhort, the mercurial powder appeared to have acted with the greateſt energy, but only within certain limits.

SECTION VII.

The effects of the mercurial powder, in the laſt experiments, made me believe that it might be confined, during its exploſion, in the centre of a hollow glaſs globe. Having therefore provided ſuch a veſſel, 7 inches in diameter, and nearly half an inch thick, mounted with braſs caps, and a ſtop-cock, (ſee Plate I.) I placed ten grains of the mercurial powder on very thin paper, laid an iron wire 149th of an inch thick acroſs the paper, through the midſt of the powder, and, cloſing the paper, tied it faſt at both extremities, with ſilk, to the wire. As the incloſed powder was now attached to the middle of the wire, each end of which was connected with the braſs caps, the packet of powder became, by this diſpoſition, fixed in the centre of the globe. Such a charge of an electrical battery was then ſent along the wire, as a preliminary experiment* had ſhown me would, by making the wire red-hot, inflame the powder. The glaſs globe withſtood the exploſion, and of courſe retained whatever gaſes were generated: its interior was thinly coated with quickſilver in a very divided ſtate. A bent glaſs tube was now ſcrewed to the ſtop-cock of the braſs cap, which being introduced under a glaſs jar ſtanding in the mercurial bath, the ſtop-cock was opened. Three cubical inches of air ruſhed out, and a fourth was ſet at liberty when the apparatus was removed to the water-tub. The exploſion being repeated, and the air all received over water, the quantity did not vary. To avoid an error from change of temperature, the glaſs globe was, both before and after the exploſion, immerſed in water of the ſame temperature. It appears, therefore, that the ten grains of powder produced four cubical inches only of air.

To continue the compariſon between the mercurial powder and gunpowder, ten grains of the beſt Dartford gunpowder were in a ſimilar manner ſet fire to in the glaſs globe: it remained entire. The whole of the powder did not explode, for ſome complete grains were to be obſerved adhering to the interior ſurface of the glaſs. Little need be ſaid of the nature of the gaſes generated during the combuſtion of gunpowder: they muſt have been carbonic acid gas, ſulphureous acid gas, nitrogen gas, and (according to Lavoiſier†) perhaps hydrogen gas. As to the quantity of theſe gaſes, it is obvious that it could not be aſcertained; becauſe the two firſt were, at leaſt in part, ſpeedily abſorbed by the alkali of the nitre, left pure after the decompoſition of its nitric acid.

SECTION VIII.

From the experiments related in the 4th and 5th ſections, in which the gunpowder proof and the gun were burſt, it might be inferred, that the aſtoniſhing force of the mercurial powder is to be attributed to the rapidity of its combuſtion; and a train of ſeveral inches in length being conſumed in a ſingle flaſh, it is evident that its combuſtion muſt be rapid. From the experiments of the 6th and 7th ſections, it is ſufficiently plain that this force is reſtrained to a narrow limit; both becauſe the block of wood charged with the mercurial powder was more ſhattered than that charged with the gunpowder, whilſt the ſand ſurrounding it was leaſt diſturbed; and likewiſe becauſe the glaſs globe withſtood the exploſion of ten grains of the powder fixed in its centre: a charge I have twice found ſufficient to deſtroy old piſtol barrels, which were not injured by being fired when full of the beſt gunpowder. It alſo appears, from the laſt experiment, that ten grains of the powder produced by ignition four cubical inches only of air; and it is not to be ſuppoſed that the generation, however rapid, of four cubical inches of air will alone account for the deſcribed force; neither can it be accounted for by the formation of a little water, which, as will hereafter be ſhown, happens at the ſame moment: the quantity formed from ten grains muſt be ſo trifling, that I cannot aſcribe much force to the expanſion of its vapour. The ſudden vaporiſation of a part of the mercury ſeems to me a principal cauſe of this immenſe, yet limited, force; becauſe its limitation may then be explained, as it is well known that mercury eaſily parts with caloric, and requires a temperature of 600 degrees of Fahrenheit to be maintained in the vaporous ſtate. That the mercury is really converted into vapour, by ignition of the powder, may be inferred from the thin coat of divided quickſilver, which, after the exploſion in the glaſs globe, covered its interior ſurface; and likewiſe from the quickſilver with which a tallow candle, or a piece of gold, may be evenly coated, by being held at a ſmall diſtance from the inflamed powder. Theſe facts certainly render it more than probable, although they do not demonſtrate, that the mercury is volatiliſed; becauſe it is not unlikely that many mercurial particles are mechanically impelled againſt the ſurface of the glaſs, the gold, and the tallow.

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* With Mr. Cuthbertſon's electrometer.

† See Lavoiſier, *Traité elementaire*, p. 527.

As to the force of dilated mercury, Mr. Baumé relates a remarkable inſtance of it, as follows:

"Un alchymiſte ſe préſenta à Mr. Geoffroy, et l'aſſura qu'il avoit trouvé le moyen de fixer le mercure par une opération fort ſimple. Il fit conſtruire ſix boîtes rondes en fer fort épais, qui entroient les unes dans les autres; la derniere étoit aſſujettie par deux cercles de fer qui ſe croiſoient en angles droits. On avoit mis quelques livres de mercure dans la capacité de la première: on mit cet appareil dans un fourneau aſſez rempli de charbon pour faire rougir à blanc les boîtes de fer; mais, lorſque la chaleur eut pénétré ſuffiſamment le mercure, les boîtes creverent, avec une telle exploſion qu'il ſe fit un bruit épouvantable: des morceaux de boîtes furent lancés avec tant de rapidité, qu'il y en eut qui paſſerent au travers de deux planchers; d'autres firent ſur la muraille des effets ſemblables à ceux des éclats de bombes *."

Had the alchemiſt propoſed to fix water by the ſame apparatus, the neſt of boxes muſt, I ſuppoſe, have likewiſe been ruptured; yet it does not follow that the exploſion would have been ſo tremendous: indeed it is probable that it would not; for, if (as Mr. Kirwan remarked to me) ſubſtances which have the greateſt ſpecific gravity have likewiſe the greateſt attraction of coheſion, the ſuppoſition that the vapour of mercury exceeds in expanſive force the vapour of water, would agree with a poſition of Sir Iſaac Newton, *that thoſe particles recede from one another with the greateſt force, and are moſt difficultly brought together, which upon contact cohere moſt ſtrongly* †.

SECTION IX.

Before I attempt to inveſtigate the conſtituent principles of this powder, it will be proper to deſcribe the proceſs and manipulations which, from frequent trials, ſeem to me beſt calculated to produce it.

100 grains, or a greater proportional quantity, of quickſilver (not exceeding 500 grains ‡) are to be diſſolved, with

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* *Chymie expérimentale et raiſonnée*, Tom. II. p. 393. *Paris*, 8vo. 1773.

† Newton's Optics, p. 372, 4th edition. *London* 1730.

‡ The reaſon of this limitation is not on account of any danger attending

heat, in a meaſured ounce and a half of nitric acid*. This ſolution being poured cold *upon* two meaſured ounces of alcohol†, *previouſly* introduced into any convenient glaſs veſſel, a moderate heat is to be applied until an effervefcence is excited. A white fume then begins to undulate on the ſurface of the liquor; and the powder will be gradually precipitated, upon the ceſſation of action and re-action. The precipitate is to be immediately collected on a filter, well waſhed with diſtilled water, and carefully dried in a heat not much exceeding that of a water-bath. The immediate edulcoration of the powder is material, becauſe it is liable to the re-action of the nitric acid; and, whilſt any of that acid adheres to it, it is very ſubject to the influence of light. Let it alſo be cautiouſly remembered, that the mercurial ſolution is to be poured *upon* the alcohol.

I have recommended quickſilver to be uſed in preference to an oxyd, becauſe it ſeems to anſwer equally, and is leſs expenſive; otherwiſe, not only the pure red oxyd, but the red nitrous oxyd, and turpeth, may be ſubſtituted; neither does it ſeem eſſential to attend to the preciſe ſpecific gravity of the acid or the alcohol. The rectified ſpirit of wine and the nitrous acid of commerce never failed, with me, to produce a fulminating mercury. It is indeed true, that the powder prepared without attention is produced in different quantities, varies in colour, and probably in ſtrength. From analogy, I am diſpoſed to think the whiteſt is the ſtrongeſt; for it is well known, that black precipitates of mercury approach the neareſt to the metallic ſtate. The variation in quantity is remarkable; the ſmalleſt quantity I ever obtained from 100 grains of quickſilver being 120 grains, and the largeſt 132 grains. Much depends on very minute circumſtances. The greateſt product ſeems to be obtained, when a veſſel is uſed which condenſes and cauſes moſt ether to return into the mother liquor; beſides which, care is to be

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ing the proceſs; but becauſe the quantities of nitric acid and alcohol required for more than 500 grains, would excite a degree of heat detrimental to the preparation.

* Of the ſpecific gravity of about 1,3.

† Of the ſpecific gravity of about ,849.

had, in applying the requiſite heat, that a ſpeedy and not a violent action be effected. 100 grains of an oxyd are not ſo productive as 100 grains of quickſilver.

As to the colour, it ſeems to incline to black, when the action of the acid on the alcohol is moſt violent, and *vice verſâ.*

SECTION X.

I need not obſerve, that the gaſes which were generated during the combuſtion of the powder in the glaſs globe, were neceſſarily mixed with atmoſpheric air; the facility with which the electric fluid paſſes through a vacuum made ſuch a mixture unavoidable.

The cubical inch of gas received over water was not readily abſorbed by it; and, as it ſoon extinguiſhed a taper, without becoming red, or being itſelf inflamed, barytes water was let up to the three cubical inches received over mercury, when a carbonat of barytes was immediately precipitated.

The reſidue of ſeveral exploſions, after the carbonic acid had been ſeparated, was found, by the teſt of nitrous gas, to contain nitrogen or azotic gas; which does not proceed from any decompoſition of atmoſpheric air, becauſe the powder may be made to explode under the exhauſted receiver of an air-pump. It is therefore manifeſt, that the gaſes generated during the combuſtion of the fulminating mercury conſiſt of carbonic acid and nitrogen gaſes.

SECTION XI.

The principal re-agents which decompoſe the mercurial powder are, the nitric, the ſulphuric, and the muriatic acids. The nitric changes the whole into nitrous gas, carbonic acid gas, acetous acid, and nitrat of mercury. I reſolved it into theſe different principles, by diſtilling it pneumatically with nitric acid: this acid, upon the application of heat, ſoon diſſolved the powder, and extricated a quantity of gas, which was found, by well known teſts, to be nitrous gas mixed with carbonic acid gas. The diſtillation was carried on until gas no longer came over. The liquor of the retort was then mixed with the liquor collected in the receiver, and the whole ſaturated with potaſh; which precipitated the mercury in a

yellowiſh-

yellowiſh-brown powder, nearly as it would have done from a ſolution of nitrat of mercury. This precipitate was ſeparated by a filter, and the filtrated liquor evaporated to a dry ſalt, which was waſhed with alcohol. A portion of the ſalt being re-fuſed by this menſtruum, it was ſeparated by filtration, and recogniſed, by all its properties, to be nitrat of potaſh. The alcoholic liquor was likewiſe evaporated to a dry ſalt, which, upon the affuſion of a little concentrate ſulphuric acid, emitted acetous acid, contaminated with a feeble ſmell of nitrous acid, owing to the ſolubility of a ſmall portion of the nitre in the alcohol.

SECTION XII.

The ſulphuric acid acts upon the powder in a remarkable manner, as already has been noticed. A very concentrate acid produces an exploſion nearly at the inſtant of contact, on account, I preſume, of the ſudden and copious diſengagement of caloric from a portion of the powder which is decompoſed by the acid. An acid ſomewhat leſs concentrate likewiſe extricates a conſiderable quantity of caloric, with a good deal of gas; but, as it effects a complete decompoſition, it cauſes no exploſion. An acid diluted with an equal quantity of water, by the aid of a little heat, ſeparates the gas ſo much leſs rapidly, that it may with ſafety be collected in a pneumatic apparatus. But, whatever be the denſity of the acid, (provided no exploſion be produced,) there remains in the ſulphuric liquor, after the ſeparation of the gas, a white uninflammable and uncryſtalliſed powder, mixed with ſome minute globules of quickſilver.

To eſtimate the quantity, and obſerve the nature, of this uninflammable ſubſtance, I treated 100 grains of the fulminating mercury with ſulphuric acid a little diluted. The gas being ſeparated, I decanted off the liquor as it became clear, and freed the inſoluble powder from acid, by edulcoration with diſtilled water; after which I dried it, and found it weighed only 84 grains; conſequently had loſt 16 grains of its original weight. Suſpecting, from the operation of the nitric acid in the former experiment, that theſe 84 grains (with the exception of the quickſilver globules) were oxalat of mercury, I digeſted them in nitrat of lime, and found my ſuſpicion juſt. The mercury of the oxalat united to the nitric acid, and the oxalic acid to the lime. A new inſoluble compound was formed; it weighed, when waſhed and dry, 48,5 grains. Carbonat of potaſh ſeparated the lime, and formed oxalat of potaſh, capable of precipitating lime-water and muriat of lime; although it had been depurated from exceſs of alkali, and from carbonic acid, by a previous addition of acetous acid. That the mercury of the oxalat in the 84 grains, had united to the nitric acid of the nitrat of lime, was proved by dropping muriatic acid into the liquor from which the ſubſtance demonſtrated to be oxalat of lime had been ſeparated; for a copious precipitation of calomel inſtantly enſued.

The ſulphuric liquor, decanted from the oxalat of mercury, was now added to that with which it was edulcorated, and the whole ſaturated with carbonat of potaſh. As effervеſcence ceaſed, a cloudineſs and precipitation followed; and the precipitate, being collected, waſhed, and dried, weighed 3,4 grains: it appeared to be a carbonat of mercury. Upon evaporating a portion of the ſaturated ſulphuric liquor, I found nothing but ſulphat of potaſh; nor had it any metallic taſte. There then remains, without allowing for the weight of the carbonic acid united to the 3,4 grains, a deficit from the 100 grains of mercurial powder, of 12,6 grains, which I aſcribe to the gas ſeparated by the action of the ſulphuric acid. To aſcertain the quantity, and examine the nature, of the gas ſo ſeparated, I introduced, into a very ſmall tubulated retort, 50 grains of the mercurial powder, and poured upon it three drams, by meaſure, of ſulphuric acid, diluted with an equal quantity of water, and extricated the gas with the aſſiſtance of a gentle heat. I firſt received it over quickſilver, the ſurface of which, during the operation, partially covered itſelf with a little black powder*.

The gas, by different trials, amounted from 28 to 31 cubical inches; it at firſt appeared to be nothing but carbonic acid, as it precipitated barytes water, and extinguiſhed a taper, without being itſelf inflamed, or becoming red. But, upon letting up to it liquid cauſtic ammoniac, there was a reſidue of from 5 to 7 inches of a peculiar inflammable gas, which burnt with a greeniſh blue flame. When I made uſe of the water-tub, I obtained, from the ſame materials, from 25 to 27 inches only of gas, although the average quantity of the peculiar inflammable gas was likewiſe from 5 to 7 inches; therefore, the difference of the aggregate product, over the two fluids, muſt have ariſen from the abſorption, by the water, of a part of the carbonic acid in its naſcent ſtate. The variation of the quantity of the inflammable gas, when powder from the ſame parcel is uſed, ſeems to depend upon the acid being a little more or leſs dilute.

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* I cannot account for this appearance.

With reſpect to the nature of the peculiar inflammable gas, it is plain to me, from the reaſons I ſhall immediately adduce, that it is no other than the gas (in a pure ſtate) into which the nitrous etheriſed gas can be reſolved, by treatment with dilute ſulphuric acid.

The Dutch chemiſts have ſhown [*], that the nitrous etheriſed gas can be reſolved into nitrous gas, by expoſure to concentrate ſulphuric acid; and that, by uſing a dilute inſtead of a concentrate acid, a gas is obtained which enlarges the flame of a burning taper, ſo much like the gaſeous oxyd of azot, that they miſtook it for that ſubſtance, until they diſcovered that it was permanent over water, refuſed to detonate with hydrogen, and that the fallacious appearance was owing to a mixture of nitrous gas with an inflammable gas.

The inflammable gas ſeparated from the powder anſwers to the deſcription of the gas which at firſt deceived the Dutch chemiſts: 1ſt, in being permanent over water; 2dly, refuſing to detonate with hydrogen; and, 3dly, having the appearance of the gaſeous oxyd of azot when mixed with nitrous gas.

The gas ſeparable by the ſame acid, from nitrous etheriſed gas, and from the mercurial powder, have therefore the ſame properties. Every chemiſt would thence conclude, that the nitrous etheriſed gas is a conſtituent part of the powder, had the inflammable and nitrous gas, inſtead of the inflammable and carbonic acid gas, been the mixed product extricated from it by dilute ſulphuric acid.

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[*] *Journal de Phyſique*, p. 250. October, 1794.

It however appears to me, that nitrous gas was really produced by the action of the dilute ſulphuric acid; and that, when produced, it united to an exceſs of oxygen preſent in the oxalat of mercury.

To explain how this change might happen, I muſt premiſe, that my experiments have ſhown me, that oxalat of mercury can exiſt in two, if not in three ſtates.

1ſt, By the diſcovery of Mr. Ameilon already quoted, the precipitate obtained by oxalic acid, from nitrat of mercury, fuſes with a hiſſing noiſe. This precipitate is an oxalat of mercury, ſeemingly with exceſs of oxygen. Mercury diſſolved in ſulphuric acid and precipitated by oxalic acid, and alſo the pure red oxyd of mercury digeſted with oxalic acid, give oxalats in the ſame ſtate.

2dly, Acetat of mercury precipitated by oxalic acid, although a true oxalat is formed, has no kind of inflammability. I conſider it as an oxalat with leſs oxygen than thoſe above mentioned.

3dly, A ſolution of nitrat of mercury boiled with dulcified ſpirit of nitre, gives an oxalat more inflammable than any other: perhaps it contains moſt oxygen.

The oxalat of mercury remaining from the powder in the ſulphuric liquor, is not only always in the ſame ſtate as that precipitated from acetat of mercury, entirely devoid of inflammability, but contains globules of quickſilver; conſequently, it muſt have parted with even more than its exceſs of oxygen; and, if nitrous gas was preſent, it would of courſe ſeize at leaſt a portion of that oxygen. It is true, that globules of quickſilver may ſeem incompatible with nitrous acid; but the quantity of the one may not correſpond with that of the other, or the dilution of the acid may deſtroy its action.

As to the preſence of the carbonic acid, it muſt have ariſen either from a complete* decompoſition of a part of the oxalat;

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* Inflammable oxalat of mercury, made to fuſe in a retort connected with the quickſilver tub, gives out carbonic acid gas.

lat; or, admitting the nitrous etheriſed gas to be a conſtituent principle of the powder, from a portion of the oxygen, not taken up by the nitrous gas, being united with the carbon of the etheriſed gas.

SECTION XIII.

The dilute muriatic acid, digeſted with the mercurial powder, diſſolves a portion of it without extricating any notable quantity of gas. The diſſolution evaporated to a dry ſalt, taſtes like corroſive ſublimate; and the portion which the acid does not take up, is left in the ſtate of an uninflammable oxalat.

SECTION XIV.

Theſe effects all tend to eſtabliſh the exiſtence of the nitrous etheriſed gas as a conſtituent part of the powder, and likewiſe corroborate the explanation I have ventured to give of the action of the ſulphuric acid. Moreover, a meaſured ounce and a half of nitrous acid, holding 100 grains of mercury in ſolution, and two meaſured ounces of alcohol, yield 90 cubical inches only of gas; whereas, without the intervention of mercury, they yield 210 inches. Upon the whole, I truſt it will be thought reaſonable to conclude, that the mercurial powder is compoſed of the nitrous etheriſed gas, and of oxalat of mercury with exceſs of oxygen.

1ſt, Becauſe the nitric acid converts the mercurial powder entirely into nitrous gas, carbonic acid gas, acetous acid, and nitrat of mercury.

2dly, Becauſe the dilute ſulphuric acid reſolves it into an uninflammable oxalat of mercury, and ſeparates from it a gas reſembling that into which the ſame acid reſolves the nitrous etheriſed gas.

3dly, Becauſe an uninflammable oxalat is likewiſe left, after the muriatic acid has converted a part of it into ſublimate.

4thly, Becauſe it cannot be formed by boiling nitrat of mercury in dulcified ſpirit of nitre, although a very inflammable oxalat is by this means produced.

5thly, Becauſe the difference of the product of gas, from the ſame meaſures of alcohol and nitrous acid, with and without mercury in ſolution, is not trifling: and, 6thly, Becauſe nitrogen gas was generated during its combuſtion in the glaſs globe.

Should my concluſions be thought warranted by the reaſons I have adduced, the theory of the combuſtion of the mercurial powder will be obvious to every chemiſt. The hydrogen of the oxalic acid, and of the etheriſed gas, is firſt united to the oxygen of the oxalat, forming water *; the carbon is ſaturated with oxygen, forming carbonic acid gas; and a part, if not the whole, of the nitrogen of the etheriſed gas is ſeparated in the ſtate of nitrogen gas; both which laſt gaſes, it may be recollected, were after the exploſion preſent in the glaſs globe. The mercury is revived, and, I preſume, thrown into vapour; as may well be imagined, from the immenſe quantity of caloric extricated, by adding concentrate ſulphuric acid to the mercurial powder.

I will not venture to ſtate, with accuracy, in what proportions its conſtituent principles are combined. The affinities I have brought into play are complicated, and the conſtitution of the ſubſtances I have to deal with not fully known. But, to make round numbers, I will reſume the ſtatement, that 100 grains of the mercurial powder loſt 16 grains of its original weight by treatment with dilute ſulphuric acid: 84 grains of mercurial oxalat, mixed with a few minute globules of quickſilver, remained undiſſolved in the acid. The ſulphuric liquor was ſaturated with carbonat of potaſh, and yielded 3,4 grains of carbonat of mercury. If 1,4 grain ſhould be thought a proper allowance for the weight of carbonic acid in the 3,4 grains, I will make that deduction, and add the remaining two grains to the 84 grains of mercurial oxalat and quickſilver; I ſhall then have,

of oxalat and mercury - - -		86 grains
and a deficit, to be aſcribed to the nitrous etheriſed gas and exceſs of oxygen -	14	
	100	

It may perhaps be proper to proceed ſtill further, and recur to the 48,5 grains, ſeparated by nitrat of lime from the 84

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* Drops of water were obſerved on the internal ſurface of the globe the day after ſeveral exploſions had been produced in its centre.

grains of mercurial oxalat and globules of quickſilver, in the 11th ſection. Theſe 48,5 grains were proved to be chiefly oxalat of lime; but they likewiſe contained a minute inſeparable quantity of mercury, almoſt in the ſtate of quickſilver, formerly part of the 84 grains from which they were ſeparated. Had the 48,5 grains been pure calcareous oxalat, the quantity of pure oxalic acid in them would, according to Bergmann[*], be 23,28 grains. Hence, by omitting the two grains of mercury in the 3,4 grains of carbonat, 100 grains of the mercurial powder might have been ſaid to contain, of pure oxalic acid 23,28 grains; of mercury 62,72 grains; and of nitrous etheriſed gas and exceſs of oxygen 14 grains. But, as the 48,5 grains were not pure oxalat, inaſmuch as they contained the mercury they received from the 84 grains, from which they were generated by the nitrat of lime, ſome allowance muſt be made for the mercury ſucceſſively intermixed with the 84 grains and the 48,5 grains.

In order to make correſponding numbers, and allow for unavoidable errors, I ſhall eſtimate the quantity of that mercury to have amounted to two grains, which I muſt of courſe deduct from the 23,28 grains of oxalic acid. I ſhall then have the following ſtatement:

That 100 grains of the fulminating mercury ought to contain,

of pure oxalic acid		21,28 grains,
of mercury formerly united to the oxalic acid	60,72	
of mercury diſſolved in the ſulphuric liquor	2	
and of mercury left in the ſulphuric liquor after the ſeparation of the gaſes	2	
total of mercury		64,72
of nitrous etheriſed gas and exceſs of oxygen		14,
		100.

Since 100 grains of the powder ſeem to contain 64,72 grains of mercury, it will be immediately inquired, what becomes of 100 grains of quickſilver, when treated as directed, in the deſcription of the proceſs for preparing the fulminating mercury.

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[*] Bergmann, *de Acido Sacchari.* Opuſcula. Tom. I. § 6. p. 248. *Leipzig*, 1788.

It has been ſtated (in ſection 9,) that 100 grains of quickſilver produce, under different circumſtances, from 120 to 132 grains of mercurial powder; and if 100 grains of this powder contain 64,72 grains, 120 grains, or 132 grains muſt, by parity of reaſoning, contain 78,06 grains, or 85,47 grains; therefore, 13,34 grains, or 20,75 grains, more of the 100 grains are immediately accounted for; becauſe 64,72 grains + 13,34 grains = 78,06, and 64,72 grains + 20,75 grains = 85,47 grains. The remaining deficiency of 21,94 grains, or 14,53 grains, which, with the 78,06 grains, or 85,47 grains, would complete the original 100, of quickſilver, remains partly in the liquor from which the powder is ſeparated, and is partly volatiliſed in the white denſe fumes, which in the beginning of this paper I compared to the *liquor fumans* of Libavius. The mercury cannot, in either inſtance, be obtained in a form immediately indicative of its quantity; and a ſeries of experiments to aſcertain the quantities in which many different ſubſtances can combine with mercury, is not my preſent object. After obſerving, that the mercury left in the reſiduary liquor can be precipitated in a very ſubtle dark powder, by carbonat of potaſh, I ſhall content myſelf with examining the nature of the white fumes.

SECTION XV.

It is clear that theſe white fumes contain mercury: they may be wholly condenſed in a range of Woulfe's apparatus, charged with a ſolution of muriat of ammoniac. When the operation is over, a white powder is ſeen floating with ether on the ſaline liquor, which, if the bottles are agitated, is entirely diſſolved. After the mixture has been boiled, or for ſome time expoſed to the atmoſphere, it yields to cauſtic ammoniac a precipitate, in all reſpects ſimilar to that which is ſeparated by cauſtic ammoniac from corroſive ſublimate.

I would infer from theſe facts, that the white denſe fumes conſiſt of mercury, or perhaps oxyd of mercury, united to the nitrous etheriſed gas; and that, when the muriat of ammoniac containing them is expoſed to the atmoſphere, or is boiled, the gas ſeparates from the mercury; and the exceſs of nitrous acid, which always comes over with nitrous ether, decompoſes the ammoniacal muriat, and forms corroſive mercurial muriat or ſublimate. This theory is corroborated by comparing the quantity of gas eſtimated to be contained in the fulminating mercury, with the quantities of gas yielded from alcohol and nitrous acid, with and without mercury in ſolution; not to mention that more ether, as well as more gas, is produced without the intervention of mercury; and that, according to the Dutch chemiſts, the product of ether is always in the inverſe ratio to the product of nitrous etheriſed gas. Should a further proof be thought neceſſary, of the exiſtence of the nitrous etheriſed gas in the fulminating mercury, as well as in the white denſe fumes, it may be added, that if a mixture of alcohol and nitrous acid holding mercury in ſolution be ſo dilute, and expoſed to a temperature ſo low, that neither ether nor nitrous etheriſed gas are produced, the fulminating mercury, or the white fumes, will never be generated; for, under ſuch circumſtances, the mercury is precipitated chiefly in the ſtate of an inflammable oxalat. Further, when we conſider the different ſubſtances formed by an union of nitrous acid and alcohol, we are ſo far acquainted with all, except the ether and the nitrous etheriſed gas, as to create a preſumption, that no others are capable of volatiliſing mercury, at the very low temperature in which the white fumes exiſt, ſince during ſome minutes they are permanent over water of 40° Fahrenheit.

[To be concluded in the next Number.]

III. *On the Origin and Progreſs of the Manufacture of Pig-Iron with Pit-Coal; and Compariſon of the Value and Effects of Pit-Coal, Wood, and Peat-Char. By Mr.* DAVID MUSHET.

AFTER the former communications, relative chiefly to the manufacturing of pig-iron with pit-coal, it may not be amiſs to make a few general obſervations as to the origin of the proceſs, the different modes of conducting it, and, by drawing a compariſon between the reſults obtained at various periods, to prove the ſlow but ſteady progreſs of improvement. A compariſon betwixt the effects produced by the uſe of charred wood and pit-coal in the blaſt-furnace will alſo afford ſubject for a few reflections.

It would appear that, towards the cloſe of queen Elizabeth's reign, blaſt-furnaces had been conſtructed of ſize ſufficient to produce, with ores and the charcoal of wood, from 2 to 3 tons *per* day, or from 15 to 21 tons of pig-iron *per* week. Such great produces in iron at this early period were, however, confined to ſituations where there was abundance of water, and where water-wheels and leathern bellows of conſiderable magnitude were uſed. The more common modes of operation were confined to furnaces of an inferior ſize, where air was ſupplied by means of bellows excited by cattle or the labour of men.

As the manufacture of cannon, mortars, &c. was at this period conſiderable, and as pit-coal had not yet been applied to any branch of the manufacturing of iron, it is probable that theſe articles would be caſt from the large blaſt-furnaces at once; the flame of wood not being well calculated for heating the large reverberating furnace where ſuch heavy pieces of ordnance are now caſt, and where ſeveral tons will be melted in one furnace by the flame of pit-coal; the non-application of pit-coal, either to the ſmelting of ores or melting of iron, would greatly retard the improvement of the ſmaller branches of the caſting department. The difficulty with which moſt Engliſh charcoal pig-iron melts, and its almoſt immediate tendency to be converted into malleable iron in an air-furnace, would, in every attempt to improvement, preſent an inſurmountable obſtacle.

This would appear, amongſt others of inferior note, to be the chief reaſon why the improvements of caſting of every denomination have only kept pace with the original invention and improvement of caſt iron with pit-coal char.

From the time of the invention by Dudley till about 60 years ago, a period of 120 years, the manufacture ſeems to have fallen aſleep: and it is only during the laſt 40 years that we can note a rapid improvement in the fabrication of caſtings. The univerſal application of pig-iron, in almoſt every poſſible ſhape, to the purpoſes of manufacture and the neceſſaries of life, wherein taſte, beauty, and durability are frequently united, belongs entirely to the preſent improved ſtate of the fabrication of pig-iron with the char of pit-coal.

In the early period above alluded to, by much the greateſt proportion of pig-iron was converted into bar-iron by means of the refinery fire, and, in many of the ſmall works, the pig-iron was *natured*, or malleabiliſed, before it was drawn from the furnace. The iron-maſters in general conſidered their own period as that of the higheſt pitch of advancement in the iron buſineſs. The erection of the great blaſt, capable, by powerful exertions, to make 21 tons weekly; the partial ſubſtitution of the refinery fire for the purpoſe of making malleable iron; and the abandonment of the hand and foot blaſt-bloweries where the other erections could be procured, were grounds ſufficient for the manufacturer to look with complacence upon the revolutions which had imperceptibly crept in, and kept pace with his practice.

The advantages which individuals derived from the manufacture of iron had induced many to engage in it. The buſineſs in point of extent ſeemed only limited to the ſupply of wood. New erections, for want of a proper ſupply of materials, became impracticable: thoſe already engaged were more anxious to preſerve their ſupply, however much circumſcribed, than to liſten to innovation, which, by ſubſtituting pit-coal for the charcoal of wood, would give to the new eſtabliſhing manufacturer a great ſuperiority in the market. It was alſo highly probable that many of the iron-works then eſtabliſhed were at a conſiderable diſtance from pit-coal, the univerſal introduction of which would have proved fatal to their intereſts. Under ſuch unfavourable circumſtances the diſcovery, or rather the practicability, of making pig-iron with pit-coal, we find announced by Simon Sturtevant, Eſq. in the year 1612, who, upon application, was favoured with a patent from king James for the excluſive manufacture of iron with pit-coal in all its branches for thirty-one years. In return, the ſaid Simon Sturtevant bound himſelf to publiſh his diſcoveries, which afterwards appeared in quarto under the title of his *Metallica*.

It is uncertain from what reaſons, but Mr. Sturtevant failed in the execution of his diſcoveries upon a large ſcale, and was obliged next year to render up his letters of monopoly.

The ſecond adventurer in this line we find to have been John Ravenſon, Eſq. who, like Sturtevant, was ſucceſsful in obtaining a patent for the new manufacture, but, like him alſo, was inadequate to the completion of it upon an extenſive ſcale. Ravenſon was alſo enjoined to publiſh his diſcoveries under the title of his *Metallica*, printed for Thomas Thorp *anno* 1613. Several other adventurers ſtepped forth, all of whom had the mortification of reſigning their patents without having contributed to the ſucceſs of the arduous undertaking.

In 1619 Dudley obtained his patent, and declared, that although he made only at the rate of three tons of pig-iron weekly, he made it with profit. The diſcovery was perfected at his father's works at Penſent in Worceſterſhire. This gentleman's ſucceſs in the various manufactures of iron with pit-coal had united not only all the proprietors of the charcoal iron trade, but many new adventurers, who wiſhed to ſhare in the emoluments of the new diſcovery. Their intereſt was ſo powerful as to limit Dudley's patent from 31 to 14 years. During the moſt of this period he continued to manufacture pig and bar-iron, and various caſtings, all of which he ſold much lower than the charcoal manufacturers. In the article of caſtings alone he muſt have had greatly the ſtart of the charcoal foundries, as the quality of carbonated coke pig-iron is far ſuperior to that of the charcoal iron of this country for the general purpoſes of caſting.

The ſuperior genius of Dudley was not always an object of paſſive indifference in the narrow eſtimation of the long eſtabliſhed manufacturers. The envy occaſioned by his uncommon ſucceſs produced at laſt a ſpirit of combination, which terminated in a hoſtile attack upon his devoted works. His improved bellows, furnace, forge, &c. all fell a prey to a lawleſs banditti, betwixt whom and its furious leaders no ſhades of diſtinction were viſible, but thoſe of avarice, ignorance, and the moſt contemptible prejudice.

To evade the mode of operation diſcovered by Dudley, or to introduce the making of pig-iron with pit-coal to greater advantage, a new plan was adopted by Captain Buck, Major Wildman, and others, in the foreſt of Dean, where they erected large air-furnaces, into which they introduced large clay pots, reſembling thoſe uſed at glaſs-houſes, filled with various proportions of the neceſſary mixture of ores and charcoal. The furnaces were heated by the flame of pit-coal, and it was expected that, by tapping the pots below, the ſeparated materials would flow out. This rude proceſs was found entirely impracticable; the heat was inadequate to perfect ſeparation, the pots cracked, and in a ſhort time the proceſs was abandoned altogether.

The misfortunes which ſucceſſively befel the unfortunate Dudley, ariſing from rivalſhip in the iron buſineſs and his attachment to the royal cauſe during the civil wars, prevented his improvements from being cloſely followed up. The refuſal of a new patent after the reſtoration prevented him from again entering into the buſineſs with his uſual enterpriſe. From that period till about the year 1740, nothing of importance was done in the manufacture of coke pig-iron. The application of the ſteam-engine for raiſing and compreſſing air, no longer confined the manufacturer to local ſituations. Larger furnaces, with a proportionate quantity of blaſt, were introduced. Among the firſt effects, from 8 to 10 tons of pig-iron were produced weekly. Ever ſince, the weekly quantity has in general been increaſing. The produce being conſiderably dependent upon the quantity of air uſed for reduction, it is now ſo well underſtood, that at ſome works the blowing-machine is calculated to produce frequently 40 tons of melting pig-iron *per* week at each furnace. At ſome ironworks in Wales, where oxygenated crude iron is manufactured purpoſely for converting into bar-iron, there are ſeveral inſtances of a furnace producing 70, 71, and 72 tons of metal weekly. This aſtoniſhing quantity forms a moſt ſtriking contraſt with the early exertions of Dudley, who conceived three tons a profitable produce, and whoſe greateſt exertions never exceeded 7 tons of pig-iron weekly.

After this ſlight ſketch of the progreſs of manufacturing pig-iron with pit-coal, it may be gratifying to make a few obſervations upon the proceſs conducted with the charcoal of wood. The ſuperior purity of the carbonaceous matter in wood, and its greater degree of inflammability, render this operation more ſimple than that performed with pit-coal. The former properly admitted of a ſmall furnace being uſed; the latter required a much leſs degree of blaſt to purify the ore, and give out ſatisfactory reſults both in the quantity and quality of the metal. Few charcoal furnaces exceeded the height of 20 feet, and many of them were from 12 to 15. A very ſmall column of blaſt was neceſſary to excite ignition, and produce the reduction of the materials. Lancaſhire and Cumberland ores were chiefly in uſe; their ſuperior richneſs in iron rendering them ſoon metalliſed when in contact with ignited charcoal of wood: 12 to 24 hours were ſufficient for this purpoſe, according to the ſize of the furnace and the quality of the pig-iron wiſhed. Not ſo in the manufacturing of coke pig-iron at preſent: the inferior quantity of iron which is contained in iron-ſtones, the impurity of the carbonaceous matter in pit-coal, eſtabliſhes a much leſs degree of affinity betwixt the metal and the principle of its reduction. The oxygen of the iron-ſtone is longer in being removed: this requires an additional period of contact. To procure this, the furnace muſt be heightened to 35 or 40 feet, and the deſcent of the materials protracted to three days.

It will be proper to exhibit the comparative effects produced by the char of pit-coal and that of wood. The following particulars will ſerve as data to make a calculation of the relative effects of the two different fuels with a charcoal furnace of 26 or 27 feet high, 9 feet wide at the boſhes, and blown by two 1¼ inch pipes placed along ſide of each other at the tuyere of the furnace. To make forge-pigs with this furnace, the following proportions have by experience been found requiſite for each charge:

2 ſacks of charcoal of 112 lb. each - -	224 lb.
7 meaſures of well dried Lancaſhire ore, each 112 lb.	784
Raw iron-ſtone - - - -	56
Limeſtone - - - - -	14

In 24 hours 18 of the above charges would have been conſumed, and nearly 3 tons of forge-pigs produced.

The total quantity of char uſed for
the quantity would be - 2 × 18 = 36 cwt.
The total quantity of iron ore - 7 × 18 = 126
——————— of iron-ſtone $\frac{1}{2}$ × 18 = 9
——————— of limeſtone - $\frac{1}{8}$ × 18 = 2$\frac{1}{2}$

We find therefore that 12 cwt. or 1344 lb. of the charcoal of wood, produces of forge pig-iron 2240 lb. or 1 ton; and that 126 cwt. of Lancaſhire ore + 9 cwt. of iron-ſtone, yielded of metal 60 cwt. This quantity, by an eaſy mode of calculation, will be found to be at the rate of 44·4 *per* cwt.

When carbonated crude iron was produced, the charge was, 200 cwt. of wood char, 5 cwt. of ore, $\frac{1}{2}$ cwt. of iron-ſtone, 14 lb. of limeſtone.

The weekly quantity was always diminiſhed in proportion to the reduction of the quantity of ore uſed. In 24 hours the quantity of the pig-iron produced, averaged 48 cwt. of carbonated crude iron. As this is the quality of metal which ſerves as the baſis of the calculation in Number XX. of the Philoſophical Magazine, it will be the moſt proper ſtandard to compare the widely different effects produced by wood and pit-coal. In that Number a table is given of the quantities of crude iron each variety of cokes produce, and which, for the ſake of immediate compariſon, I ſhall here again inſert.

1 ton, or 2240lb. clod-coal cokes produces of carbonated iron - -	1040 lb.
1 ton, or 2240lb. ſplint-coal cokes -	840
——— 2240 mixed coals - -	702
1 ton, or 2240lb. of charcoal of wood, according to the proportions furniſhed above, will produce of carbonated iron -	2986
1 ton, or 2240lb. of the ſame, will produce of oxygenated crude iron for forge-pigs	3718; and
1 ton of carbonated pig-iron will require of the cokes of clod-coal - -	4824.6 lb.
——————— ſplint-coal -	5973·3
——————— mixed -	7147·5
——————— wood charcoal -	1680

From this comparative view it is found that charcoal of wood produces triple effects, or carbonates three times the quantity of crude iron that the clod-coal cokes do; $3\frac{1}{2}$ times as much as the ſplint; and $4\frac{1}{2}$ times as much as a mixture of free and ſplint.

The next conſideration is the price of the two fuels. Charcoal of wood, about forty years ago, ſold at 2*s.* 6*d.* *per* ſack of 1 cwt. or 50*s.* *per* ton. 1 ton of good ſplint-coal cokes will be prepared on many of the preſent iron-work banks, labour included, at 11*s.* and at ſome places ſo low as 10*s.* At preſent the price of charcoal is upwards of 4*l.* *per* ton. So that at this period, although the effects of wood are $3\frac{1}{2}$ times thoſe of ſplint-coal cokes, yet the price of one ton of charcoal wood will purchaſe eight tons of cokes; the quantity of the former limited to, and produced from, land which might be better applied to the purpoſes of agriculture; the latter found in immenſe fields, and in tracts of country which are always augmented in value by the development of their mineral treaſure.

A charcoal blaſt-furnace which ſmelts the whole year round, and occaſionally makes forge-pigs and carbonated iron to the amount of 1000 tons annually, will conſume 14,000 ſacks of charcoal; which may be eſtimated at 1 cwt. *per* ſack, or 700 tons, or 1,568,000 lb. This divided by 18.75 lb. the pounds in a cubic foot, gives for the quantity of timber in cubical feet, 83.626. This is going upon a former calculation, and is ſuppoſing the wood to ſhrink but little during the proceſs of charring. In the preſent ſtate of the woods which are attached to iron-works, one acre will not yield more than 1200 cubical feet of timber: To enſure the annual ſupply, $\frac{83626}{1200} = 69.69$ acres of land would require every year to be cleared, or nearly 1400 acres would be requiſite to form, with proper care, an unfailing ſource of ſupply; and at the rate of 4*l.* *per* ton, the fuel would coſt 2,800*l.*

Let this be compared with a blaſt-furnace manufacturing the ſame quantity and quality of coke pig-iron. The average quantity for each ton will be nearly ſix tons of ſplint, or, as they loſe 50 *per cent.* in charring, 3 tons of cokes

× 1000 tons of pigs = 3000 tons of cokes, which at the higheſt price ſtated, 11*s.* *per* ton, amounts to 1,650*l.* or leſs than the charcoal, 1,150*l.* 6000 tons of ſplint-coals will be eaſily procured from *one* acre of meaſurement where the ſeam meaſures 4½ to 5 feet thick. An obſervation ſtrikes us forcibly here, that, great as the conſumption of wood is, a ſufficient extent of country can from time to time replace any given quantity. No facts, however, which have hitherto come under our obſervation, warrant us to ſuppoſe the reformation of pit-coal.

I cannot cloſe this paper without bringing forward to notice a ſubſtance which has hitherto remained in a comparatively uſeleſs ſtate to ſociety. We have ſeen how many extenſive foreſts of excellent land have been cleared of their natural timber; how many thouſand acres reſtored to agricultural purpoſes, to ſupply the wants of the increaſing population of our country by the immediate neceſſities of the iron-works; while at the ſame time individuals received ample recompenſe for their neceſſary labour. May we not again look forward and truſt, that the rapid ſtrides which ſcience in our times is making towards the perfection of manufactures, will deviſe a method which will enable the manufacturer of iron to uſe peat and turf in his manipulations, either alone or as a mixture? Many ſchemes have been tried to introduce theſe cheap fuels to advantage. Mixtures of charcoal and well dried peats ſmelt and carbonate a conſiderable portion of metal. The weekly produce, however, ſeldom exceeded eight or ten tons; the extreme inflammability of the peat rendered it impracticable to uſe a blaſt ſufficiently compreſſed to produce a quantity without riſking the furnace. When peats were uſed alone, the produce diminiſhed ſo much as not to defray the labour. But the quality of the metal was always ſuperior, and nothing was wanting but a preparation of the fuel in ſuch a manner as to ſtand a heavier diſcharge of air.

Peats have alſo been uſed for the refining of caſt into bar-iron with various degrees of ſucceſs, none of them ſufficiently important in their advantages to conſtitute a permanent ſubſtitute for pit coal or the charcoal of wood. The qualities of peat-ground, or rather of the moſs from which peats are cut, are as various as thoſe of pit-coal. That which ſeems to have undergone the moſt complete decompoſition, to have become denſe and compactly united in all its parts, forms the hardeſt and moſt durable peat. The blacker the colour, the more carbonaceous is the fuel, and the firmer is the char it produces. In moſs-grounds where the decompoſition is not complete, the peat abounds more with earths and bog-iron ore. The remains of the vegetables are more entire, and its tendency to conſolidation in burning is leſs. I have expoſed ſuch peat as this to a long drying heat, and found 100 parts of it, when thus dried, to be compoſed of,

Water, hydrogen, and ſulphur	72.8
Carbon	15.1
Oxyd of iron, with a ſlight mixture of alkaline ſalts	12.1
	100 parts.

The ſuperior quality of peat or turf hardens by long keeping, and becomes leſs fragile than common pit-coal, and poſſeſſes alſo conſiderable weight. The following may be conſidered as an analyſis of what is commonly found in Lanarkſhire:

Water, hydrogen, and ſulphur	72.6
Carbon	25.2
Browniſh magnetic aſh, very light, and conſiderably alkaline	2.2
	100 parts.

The above quality of peats forms, by careful ignition, a char as firm as ſome varieties of wood: and it is further thought poſſible, that the mode of charring may be ſo varied as, by cauſing a part of the carbon to diſſolve in the hydrogen, to form a bituminous cement for the whole, and render the char equal in compactneſs to that of wood.

The expenſe of caſting peats is the next object which ought here to be conſidered. A man will with eaſe caſt 4000 of an ordinary ſize *per* day. For this quantity are required two carriers, commonly women, and one dyker*.

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* A perſon employed to pile up the peats to dry.

The peat, when thoroughly dried, will weigh 3 pounds; the product, therefore, will be 3 × 4000 = 12,000 lb. upwards of 5 tons. The total expenſe of this quantity for this ſhare of labour would be,

The man's wages - -	2*s.* 6*d.*
Two women ditto, 1*s.* each	2*s.* 0*d.*
Girl for dyking - -	0*s.* 6*d.*
Or equal to 1*s.* *per* ton.	5*s.* 0*d.*

Suppoſe that with additional labour, and the trouble of drying, a ſum equal to the firſt was incurred, ſtill we find that peats may be furniſhed in great plenty at 2*s.* *per* ton. By the former analyſis it appears that 4 tons would do more than make one ton of peat char, containing a portion of vegetable carbon ſuperior even to that contained in oak. This quantity of char would only coſt at moſt 8*s.* *per* ton, beſides the labour of charring, which, by proper erection and execution, would not coſt more than 2*s.* 6*d.* making in all 10*s.* 6*d.* The charcoal wood in this country at preſent ſells at the enormous price of 4*l.* 6*s.* 8*d.* *per* ton, and is ſcarce in point of quantity. Should it ever happen that peat moſs ſhall become a ſubſtitute for pit-coal, or obtain a preference from local advantages and cheapneſs by the manufacturers of iron, we ſhall then ſee a happy commencement to the moſt rational and moſt likely way of regenerating immenſe heaths of country, and of rendering up large tracts of ſurface, which for ages have remained uſeleſs to the wants of man; this way conquering one of the moſt pernicious evils which the progreſs of agriculture ever encountered.

It will, however, moſt likely be found, that a procedure upon ancient modes of manufacture already eſtabliſhed, will be productive of no greater ſucceſs than has hitherto been experienced. It is the buſineſs of the manufacturer to apply his knowledge of the principle of his proceſs to the exigency of the ſtructure and component parts of one or all of his materials. The general introduction of peat or turf for the manufacture of iron in preferable ſituations in our age, ought not to excite the ſame degree of wonder that Dudley's invention with pit-coal did in the laſt century. Thus, it is very poſſible that the bare attempt will meet with as keen reprobation, dictated by the ſame narrow, ſelfiſh, and unenlightened ſpirit which marked that period, and which has hitherto clouded the genius of our manufacturing claſs.

IV. *Advantageous Method of ſeparating the whole of the tartarous Acid from crude Tartar. By M.* LOWITZ; *abridged by Dr.* VAN MONS, *of Bruſſels**.

MR. LOWITZ ſhowed, in the year 1786, that a tartarous acid might be obtained by charcoal duſt from crude tartar, as pure as that obtained from white tartar; but as the tartrite of potaſh, which is ſeparated in this operation, is too ſtrongly united to the colouring matter of the tartar to be able to get rid of it, the loſs of this ſalt, neceſſarily occaſioned by the proceſs, deſtroyed all the advantage derived from it. Means, indeed, had been found to decompoſe entirely the acidulous tartrite of potaſh by the means of quicklime; but this proceſs is accompanied with great inconveniencies, which M. Goettling attempted to obviate by propoſing to decompoſe the tartrite by the acetite of the ſame earth.

This proceſs, however, of Goettling cannot be employed but with white tartar, on account of the impoſſibility of depriving of its colour the acetite of potaſh which reſults from it. This induced M. Lowitz to endeavour to diſcover another method of decompoſition.

This method conſiſts in mixing together 15 lb. of crude tartar with 4 lb. of chalk, and adding gradually 200 lb. of cold water. After the effervefcence has ceaſed, the mixture muſt be heated; and at the moment when it boils, you throw into it ſometimes a little tartar, and ſometimes a little chalk, until no more effervefcence is excited: you then filter the liquid, and precipitate it by means of a ſolution of the muriat of lime. You then decant the coloured liquid, and waſh the united precipitates until they have been deprived of all the ſeparable part of their colour, and until they become inſipid. This tartrite of lime is then decompoſed by eight pounds of ſulphuric acid diluted with as much water; and the acid ſeparated, is again diluted with 50 or 60 pounds of that liquid. It is neceſſary that the acid ſhould contain a ſlight exceſs of ſulphuric acid. The liquor is then ſeparated from the ſulphat of lime; and after it has been mixed with from four to ſix pounds of charcoal duſt, it is evaporated to the point of cryſtalliſation by being kept in a continual ſtate of ebullition. When the liquor has cooled, you ſeparate from it a ſmall remnant of the ſulphat of lime which has been depoſited, and you ſubject it to inſenſible evaporation to make it cryſtalliſe.

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* From the *Annales de Chimie*, No. 101.

M. Lowitz mentions a mode of aſcertaining the juſt proportion of the free ſulphuric acid which the liquor ought to contain in order to prevent a portion of the tartrite of lime from remaining diſſolved in the ſtate of acidulous tartrite, which cauſes the tartarous acid, inſtead of cryſtalliſing, to conſolidate itſelf into a ſaline maſs, without form, and pulverulent. This method conſiſts in diluting in an ounce of water half a dram (gros) of the acid ſolution, and adding from 10 to 15 drops of the liquid acetite of lead. On dropping nitric acid into this mixture, if the liquid immediately becomes tranſparent, and remains ſo for ſeveral hours, it is a proof that there is too little acid: if the liquid does not immediately become clear, there is too much: in the laſt place, if, after having become clear, it again grows turbid at the end of a few minutes, the juſt point has been attained. This trial ought to be made at the time when the liquor has reached the point of ſaturation to be expoſed to cryſtalliſe.

This operation will furniſh eight pounds of tartarous acid in very beautiful cryſtals, and exceedingly white. To free it from any of the ſulphuric acid which may be adhering to it, nothing will be neceſſary but to throw over it a little cold water.

We would adviſe thoſe who wiſh to follow this proceſs, to dilute the ſulphuric acid with at leaſt four times its weight of water, in order to prevent the de-hydrogenating action of that acid on the tartarous acid; not to dilute the acid with the whole quantity of water which the author preſcribes, in order that the evaporation may be ſhortened; not to continue the ebullition when the liquor has acquired a certain degree of concentration, with a view to prevent the de-hydrogenating effect of the heat; and, in the laſt place, not to add an exceſs of ſulphuric acid till the whole of the ſulphat of lime, which this acid in acidulating would render ſoluble, is ſeparated.

M. Lowitz juſtly obſerves, that the ſalt which he employs to decompoſe the tartrite of potaſh is found in all the laboratories of druggiſts as the remains of other operations, and that it is often thrown away as uſeleſs.

The author propoſes to draw tartarous acid in the ſame manner, from tartrite of lime, which is precipitated in great abundance after the firſt cryſtalliſations of the acidulous tartrite of potaſh and of ſoda, and which remains in the mother-water of that ſalt.

M. Lowitz terminates this memoir with adviſing to leave in the citric acid, as in the tartarous acid, a ſlight exceſs of the ſulphuric acid; which is the more neceſſary in it, as the acidulous citrat of lime is much more ſoluble than the tartrite of that earth. M. Lowitz aſſures us that this practice contributes much towards obtaining the citric acid in large and regular cryſtals.

V. *On the Method of preparing Ether by the Muriatic Acid, or the Marine Ether of the Shops. By* J. B. Van Mons[*].

THE preparation of ether by the muriatic acid is a proceſs which could not be brought to perfection till after the diſcovery of the oxygenated muriatic acid, and which required to have light thrown upon it by the new chemical doctrine of the French. It is not aſtoniſhing, therefore, that the efforts of the old chemiſts ſhould have often miſcarried in the preparation of this liquid.

Indeed, as the ſuppoſed radical of the ſimple muriatic acid, and the unoxygenated combinations of that acid, do not part with oxygen to any ſubſtance or union of known combuſtible ſubſtances; and, as etherification is an effect which depends on the action of this principle on the ſtate of the hydro-carbonous combination of alcohol, the converſion of the alcoholic liquor into ether could be hoped for only through the intervention of this principle. Ludolph[*], Baumé, and others, attempted to make marine ether by means of common muriatic acid; but without ſucceſs, notwithſtanding the concentrated and active ſtate in which they have applied this acid on alcohol[†].

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[*] Read in the Medical Society at Bruſſels, 30 Nivoſe, an. 8.

Some chemiſts, however, without knowing that ſtate of combination of oxygen with the muriatic acid which conſtitutes the oxygenated acid, (probably with a view to preſent to the alcohol a highly concentrated acid,) employed for making marine ether different oxygenated metallic muriats. Thus, Baſil Valentine, and after him Wenzel, tried with that view the oxygenated muriat of antimony; Neumann, Bayen, de Bormes, the ſame muriat of zinc; Pott, that of arſenic; Rouelle, Courtanvaux, Spielmann, Erxleben, and lately the celebrated Klaproth[‡], that of tin; and others the oxygenated muriat of mercury, the red muriat of iron, &c. and the reſults have been attended with more or leſs ſucceſs; but it was neceſſary, as already ſaid, that the real preparation of this ether ſhould be preceded by the diſcovery of the oxygenated muriatic acid.

The different proceſſes, however, for the preparation of marine ether by this acid are ſtill imperfect, and, for the moſt part, betray inexperience and ſervile imitation. Scheele did not fail to apply ſoon the wonderful properties which he had diſcovered in this acid (a diſcovery by which he has been immortaliſed) to the preparation of the liquid in queſtion[*]. Beſides other proceſſes, he diſtilled muriatic acid from off the oxyd of manganeſe; received the oxygenated acid in a veſſel containing alcohol; and rectified the liquid, thus impregnated and etherified, by a gentle heat. But theſe operations only furniſhed hints for conducting chemiſts to a better proceſs; for Hahnemann[†], Weſtrumb[‡], and Scheele himſelf agree, that by means of the oxygenated acid they obtained rather a vinous oil than real ether.

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[*] (Ludolph) Einleitung in di Chim. 1664. (Baumé) Diſſertation ſur l'Ether, 1767.

[†] Brugnatelli, no doubt, had heard mention made of the oxygenated acid, when he ſaid, that the muriatic acid with alcohol produced a perfect marine ether: "*l'oſſi muriatico ſomminiſtra coll' alcoole, un perfetto etere di oſſi muriatico.*" Elementi di Chimica, 1796, Vol. II. p. 56.

[‡] (Valentin) Tolden, Letztes Teſtament, 1712, p. 218; (Wenzel) Lehre von der Verwandtſchaft, p. 148; (Neumann) Prælect. Chim. Leip. 1737; (Bayen) Mém. de l'Acad. des Sciences 1774, Vol. IV; (De Bormes) Mém. des Savans Etrangers, Vol. VI. p. 612; (Pott) Obſ. Chim. de Acid. Sal. Vinoſ. 1739, p. 117; (Rouelle) Journal des Savans 1759; (Courtanvaux) Mém. des Savans Etrangers, Vol. VI; (Spielmann) Inſtitut. Chim. 1763; (Erxleben) Anfangſgr. der Chem. 1775, p. 272; (Klaproth) Von Crell's Chem. Annal. 1796, Vol. I. p. 20.

Pelletier[§], and before him Weſtrumb, as well as other German chemiſts, adopted the proceſs in which Scheele preſcribed the diſtillation of alcohol from a mixture of the muriat of ſoda, ſulphuric acid, and the oxyd of manganeſe: but this manipulation, in the ſtate of imperfection in which it has hitherto been performed, either gave a little ether by the action of the ſulphuric acid, or furniſhed only the produce of the other proceſſes employed by the Swediſh chemiſt. I ſhall enter into ſome details reſpecting the phenomena which take place in theſe operations.

It is impoſſible that this liquid, by the action of the oxygenated muriatic acid on the alcohol, ſhould not experience, in a degree proportioned to its principles, that change from which ether reſults. This, indeed, is what has been obſerved by thoſe who put in practice the methods of Scheele and Weſtrumb. They firſt ſaw the liquid float on the ſurface of the water; but when they wiſhed to ſubmit it to rectification, or when they did not ſeparate it ſoon enough from the water impregnated with the oxygenated acid, they found their ether converted into oil, firſt liquid and floating on the water, when they ſeparated it from the alcohol in which it was diſſolved; but afterwards becoming thick, and falling to the bottom of the ſame liquid. This olefication of ether is a natural effect of the oxygenating action of the acid, when care has not been taken to ſtop this operation immediately after the ether is formed. A like action is exerciſed by the air of the atmoſphere on natural oils.

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[*] Koning. Vetenſkaps-Academiens nya Handlingar, 1782, Vol. III. p. 35.

[†] Phyſ. Chem. Abhand, Vol. I. p. 35.

[‡] Laborant, in Groſſen, Vol. I. p. 236.

[§] Mem. de Chimie, Vol. I. p. 39.

The oxygenated muriatic acid, which paſſes at the ſame time as the ether, continues to act on that liquid, and makes it undergo a modification of compoſition that brings it to the ſtate of oil, to which it had before nearly approached. This effect, as I have ſaid, takes place when the ether is left for ſome time mixed with the acid it has carried along with it, or when it is rectified without ſeparating it from that acid. It may readily be conjectured, that the continuation of the ſame labour ought to make the liquid oil paſs to the ſtate of thick oil, and that it would bring the latter to the ſtate of greaſe, and, conſequently, to that of wax. The ſame thing takes place with the olefying gas of the Dutch chemiſts, which from ethereous gas transforms itſelf, by the re-action of the oxygenated muriatic acid gas on the ſtate of the combination of its principles, into an oil, firſt liquid and floating on the water, but which afterwards becomes inſpiſſated to olefication when you add only a few bubbles of the latter gas in exceſs. When this exceſs of oxygenated gas was conſiderable, I have often ſeen the ethereous oil converted into a real white greaſe, opake, and of the conſiſtence of half melted tallow. I informed the public, more than ten years ago, that the oxygenated muriatic acid gas, kept over oil of *colſat* (coleſeed), converted it into a waxy matter, taking from it its ſmell and colour, and rendering it perfectly white. All theſe effects depend on withdrawing a portion of the hydrogen; ſo that ether is oil, plus a certain portion of hydrogen; oil is greaſe, plus that proportion, &c. Ether made by the ſulphuric acid undergoes, but more ſlowly, the ſame transformation into oil, in the like manner as that made by the nitric acid, which experiences this change almoſt as ſpeedily as muriatic ether does. Nothing is ſo eaſy to be conceived as the de-hydrogenation, at all temperatures, effected by condenſed oxygen.

Thoſe who have obſerved this ready olefication of marine ether muſt have given a preference, the motives of which are not difficult to be diſcovered, to a certain proceſs which preſcribes that the alcohol ſhould be mixed with the ſulphuric acid before this acid is poured over the two other ingredients, muriat of ſoda and oxyd of mangaueſe. There is formed by the aid of the heat which is excited in this mixture, as Fourcroy and Vauquelin obſerved, a quantity of ether by the ſulphuric acid, which firſt paſſes in the diſtillation, and which ſuffers itſelf to be ſeparated with more certainty, being leſs liable to be attacked by the oxydating principle than marine ether. This ſulphuric ether muſt have often been conſidered by inexperienced judges as real muriatic ether; from which, however, it differs by a ſmell and taſte peculiar to itſelf.

In this ſtate of the ſcience reſpecting the preparation of marine ether, I have thought it my duty to communicate to the Society a proceſs which removes every difficulty in this preparation.

Place in a ſand-bath, gently heated, the retort of Woulfe's apparatus as improved by Lavoiſier, and compoſed only of a balloon and two bottles: introduce into it 1.00 of any weight of the muriat of ſoda, perfectly dry; and in the receiver and the two bottles diſtribute the ſame weight of good alcohol. After having exactly luted the joinings, and furniſhed the laſt bottle with a tube of ſafety, pour upon the ſalt in the retort 0·50 of concentrated ſulphuric acid, and leave the operation to proceed cold for five or ſix hours. Then make a moderate fire, which muſt be augmented by degrees, to bring the bottom of the capſule of the bath to a ſlight degree of red heat. The muriatic acid in the natural ſtate is gaſeous: in that ſtate it paſſes over and diſſolves in the alcohol. It is of benefit in this operation to plunge the tubes of communication to a certain depth in the alcohol, that the gas may experience a condenſation, which greatly contributes to its abſorption. This diſpoſition of the tubes, when the diſengagement is rapid, makes the alcohol ſometimes paſs from the one bottle into the other; in which caſe the poſition of the bottles muſt be changed in ſuch a manner as to make that which contains the greater quantity of liquid to communicate with the receiver. It may readily be conceived that this paſſage of the alcohol from one bottle to another, and even its elevation to ſome centimetres above its level, is neceſſarily the effect of a conſiderable degree of compreſſion experienced by the gas, and which re-acts, or is tranſmitted by the latter to the liquid; for, even by applying to the explanation of the phenomena of a liquid in motion the general principle of mechanics, that the maſſes and velocities are inverſely proportional, it is nevertheleſs true, that at reſt the force which raiſes and maintains a liquid above its level, cannot be inferior to the weight of a maſs of that liquid having for height its elevation in the tube, and for baſe the internal diameter of the bottle; or, to make uſe of common language, having for bulk its elevation multiplied by its diameter: but a compreſſing force, to be exerciſed by the gas, ought to be experienced by it; and this compreſſion, by concentrating its parts, diſpoſes it to diſſolve in the alcohol. I have indulged in this ſhort digreſſion, which may appear foreign to my object, on account of its numerous applications in the practice of the chemiſtry of gaſes.

When the whole muriatic acid has paſſed, the liquors in the different bottles are to be united, and poured into the retort, after firſt taking out the ſalt. It forms a muriatic alcohol exceedingly concentrated. You muſt add in the retort 0·20 of the oxyd of mangaueſe in very fine powder; and put into the recipient, and the two bottles, a certain quantity of a ſolution of cauſtic potaſh. You then diſtil at a gentle heat, conducted with care. It is here ſeen that the alkaline liquor ſerves to enchain the oxygenated acid which is in exceſs at the formation of the ether, and that it prevents the action which oleſies this liquid. It is chiefly in this practice that the part of my method which preſerves the ether conſiſts.

Notwithſtanding this precaution, it is not poſſible, however, to prevent a greater or leſs portion of the ether from being decompoſed by the acid, which muſt traverſe that liquid to reach the alkaline liquor: beſides, the oxygenated muriatic acid united to the alkali, continues no leſs to act as an oxygenating ſubſtance in its ſtate of muriat; for, properly,

perly, it is the action of the acid, and not that of the oxygen, which the alkali neutraliſes. I have freed the latter entirely from theſe inconveniencies by receiving the ether on liquid ammonia, which is burnt by the oxygen in proportion as it comes in contact with it: but this manipulation is accompanied with too many dangers for me to venture to recommend it in general.

Another method of ſeparating the ether from the oxygenated acid would be, to receive this liquid on water; to remove it in proportion as it floats on the ſurface, by means of a ſyphon or pump; to ſhake it with the alkaline ſolution; and to decant it again on pure water. You then obtain the whole ether, which may be afterwards ſeparated from the water by rectification.

Ether, freed in any manner from its acid, muſt finally be mixed with twice its bulk of water, and rectified at a heat equal to that of Guyton's economical furnace.

A marine ether may be compoſed by one operation if you diſtill, at a boiling heat, a mixture of alcohol and the oxygenated muriat of potaſh in the proportion of 1·00 to 0·25.

It is not neceſſary to recommend to thoſe who are ever ſo little habituated to this kind of labour, to cool, from time to time, in the courſe of the diſtillation, the receiver, by applying to it a wetted ſponge.

VI. *Obſervations upon an unuſual horizontal Refraction of the Air; with Remarks on the Variations to which the lower Parts of the Atmoſphere are ſometimes ſubject. By the Rev.* S. VINCE, *A.M. F.R.S. and Plumian Profeſſor of Aſtronomy and experimental Philoſophy in the Univerſity of Cambridge*[*].

THE uncertainty of the refraction of the air near the horizon has long been known to aſtronomers, the mean refraction varying by quantities which cannot be accounted for from the variations of the barometer and thermometer; on which account, altitudes of the heavenly bodies which are not more than 5° or 6° ought never to be made uſe of when any conſequences are to be deduced from them. The cauſe of this uncertainty is probably the great quantities of groſs vapours, and exhalations of various kinds, which are ſuſpended in the air near to the earth's ſurface, and the variations to which they are ſubject; cauſes, of which we have no inſtruments to meaſure the effects which they produce in refracting the rays of light. In general, the courſe of a ray paſſing through the atmoſphere, is that of a curve which is concave towards the earth, the effect of which is to give an apparent elevation to the object; and thus the heavenly bodies appear above the horizon, when they are actually below it; but it will not alter the poſition of their parts in reſpect to the horizon; that is, the image of the higheſt part of the object will be uppermoſt, and the image of the loweſt part will be undermoſt. The figures, however, of the ſun and moon, when near the horizon, will ſuffer a change, in conſequence of the refraction of the under limb being greater than that of the upper; from which they aſſume an elliptical form, the minor axis of which is perpendicular to the horizon, and the major axis parallel to it. But a perpendicular object, ſituated upon the ſurface of the earth, will not have its length altered by refraction, the refraction of the bottom being the ſame as that of the top *. Theſe are the effects which are produced upon bodies at or near the horizon, in the common ſtate of the atmoſphere, by what I ſhall call the *uſual* refraction.

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[*] From the *Tranſactions of the Royal Society for* 1799.

But, beſides the uſual refraction which affects the rays of light, the atmoſphere over the ſea is ſometimes found to be in a ſtate which refracts the rays in ſuch a manner as to produce other images of the object, which we will call an effect from an *unuſual* refraction. In the Philoſophical Tranſactions for 1797, Mr. Huddart has deſcribed ſome effects of this kind, which he has accounted for by ſuppoſing that, from the evaporation of the water, the refractive power of the air is not greateſt at the ſurface of the ſea, but at ſome diſtance above it; and this will ſolve, in a very ſatisfactory manner, all the phænomena which he has obſerved. But effects very different from thoſe which have been deſcribed by Mr. Haddart are ſometimes found to take place. Theſe I had an opportunity of obſerving at Ramſgate, laſt ſummer, on Auguſt the firſt, from about half an hour after four o'clock in the afternoon till between ſeven and eight. The day had been extremely hot, and the evening was very ſultry; the ſky was clear, with a few flying clouds. I ſhall deſcribe the phænomena as I obſerved them with a terreſtrial teleſcope, which magnified between 30 and 40 times; they were viſible, however, to the naked eye. The height of the eye, above the ſurface of the water, at which moſt of the obſervations were made, was about 25 feet; ſome of them, however, were made at about 80 feet from the ſurface; and it did not appear that any of the phænomena were altered from varying the height of the eye, the general effect remaining the ſame.

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* See my *Complete Syſtem of Aſtronomy*, art. 194.

The firſt unuſual appearance which I obſerved, was that which is repreſented in Plate II. fig. 1. Directing my teleſcope at random, to examine any objects which might happen to be in view, I ſaw the top of the maſts of a ſhip A, above the horizon, *x y*, of the ſea, as ſhown in the figure; at the ſame time alſo, I diſcovered in the field of view, two complete images, B, C, of the ſhip in the air, vertical to the ſhip itſelf, B being inverted, and C erect, having their hulks joined. The phænomenon was ſo ſtrange that I requeſted a perſon preſent to look into the teleſcope and examine what was to be ſeen in it, who immediately deſcribed the two images, as obſerved by myſelf; indeed they were ſo perfect, that it was impoſſible we could differ in our deſcription. Upon this, I immediately took a drawing of the relative magnitudes and diſtances of the ſhip and its images, which, at that time, were as repreſented in the figure, as near as it was poſſible for the eye to judge; and it was very eaſy to eſtimate them to a very conſiderable degree of accuracy. As the ſhip was receding from the ſhore, leſs and leſs of its maſts became viſible; and, continuing my obſervations, in order to diſcover whether any, or what variations might take place, I found that, as the ſhip deſcended, the images B, C aſcended;

but, as the ſhip did not ſink below the horizon, I had not an opportunity of obſerving at what time, and in what order, the images would have vaniſhed, if the ſhip had ſo diſappeared.

Being deſirous of ſeeing whether the ſame effect was produced upon the other ſhips which were viſible, I directed my teleſcope to another ſhip A, (Fig. 2.) whoſe hulk was juſt in the horizon *xy*; when I obſerved a complete inverted image B, the main-maſt of which juſt touched that of the ſhip itſelf. In this caſe, there was no ſecond image as before. The ſhip A moving upon the horizon, B continued to move with it, without any variation in its appearance.

The next ſhip which I directed my teleſcope to, was ſo far on the other ſide of the horizon *xy*, as juſt to prevent its hulk from being ſeen, as is repreſented by A, (Fig. 3). And here I obſerved only an inverted image of part of the ſhip, the image *y* of the topſail, with the maſt joining that of the ſhip, the image *x* of the top *a* of the other maſt, and the image *z* of the end *c* of the bowſprit, only appearing at that time. Theſe images would ſuddenly appear and diſappear very quickly after each other; firſt appearing below, and running up very rapidly, ſhowing more and leſs of the maſts at different times, as they broke out; reſembling, in the ſwiftneſs of their breaking out, the ſhooting out of a beam of the Aurora borealis. As the ſhip was deſcending on the other ſide of the horizon, I continued my obſervations upon it, in order to diſcover what changes might take place; when I found that, as it continued to deſcend, more of the image gradually appeared, till at laſt the image of the whole ſhip was completed, with their main-maſts touching each other; and, upon the ſhip deſcending lower, the image and the ſhip ſeparated; but I obſerved no ſecond image, as in the firſt caſe: a ſecond image, however, might probably have appeared if the ſhip had continued to deſcend.

Upon moving my teleſcope along the horizon, in order to examine any other ſhips which might be in ſight, I obſerved, juſt at the horizon *xy*, (in Fig. 4.) the top *a* of the maſt of a ſhip; and here an effect was obſerved which had not been before diſcovered; for there was an inverted image B, vertical to *a*, an erect image C, both of them very perfect and well defined, and an image $v\,w$ of the ſea between them, the water appearing very diſtinctly. As the ſhip was coming up towards the horizon, I continued to obſerve it, in order to diſcover the variations which might follow, and found, that as the ſhip approached the horizon, the image C gradually diſappeared, and at laſt it vaniſhed; after that, the image $v\,w$ of the ſea diſappeared; and during this time the image B deſcended; but the ſhip did not riſe ſo near to the horizon as to bring the main-maſts together. Had I directed my teleſcope to the ſame point of the horizon a little ſooner, I ſhould have ſeen the two images before the ſhip itſelf was viſible. In fact, the images were viſible when the whole ſhip was actually below the horizon; for, from the very ſmall part of the maſt which was at firſt viſible, that part muſt then have been below the horizon, and appeared above it by the uſual refraction; the altitude of *a*, above the horizon, having then been much leſs than the increaſe of altitude which ariſes from the common horizontal refraction. The diſcovery of ſhips in this manner might, in ſome caſes, be of great importance; and, on ſuch occaſions, it might be worth while to appoint proper perſons to make obſervations for that purpoſe.

The cliffs at Calais being very viſible, I directed my teleſcope towards them, in order to examine whether there was any thing unuſual in their appearance; when I obſerved an image of the cliffs above the cliffs themſelves, together with an image of the ſea ſeparating them, as is repreſented in Fig. 5.; in which, $x\,y$ repreſents the horizon of the ſea, AB the cliffs, $a\,b$ their image, and $v\,w$ the image of the ſea between them: the depth of $a\,b$ was much leſs than that of AB. It is probable, however, that $v\,w$ might not be the image of the ſea immediately adjoining to the cliffs, but a partial elevation of the ſea at ſome diſtance from them; and that the image $v\,w$ might intercept ſome part of the image $a\,b$, which would otherwiſe have been viſible: we muſt not, therefore, conclude, that the image $a\,b$, ſo far as it appeared, was leſs than the correſponding part of the object. From the memorandums which I made at the time of obſervation, I do not find that I examined the appearance of the cliff AB, and its image *a b*; which, had there at that time been any ſtriking marks in them, would have determined whether the object and its image were of the ſame magnitude. The image *a b* was, however, erect; the boundaries on the top of AB and *a b* agreeing together. Having examined this for ſome time, and taken a drawing of the appearance, during which I could diſcover no variation, I directed my teleſcope to other objects; and, upon turning it again to the ſame cliffs, after the ſpace of about ſix or ſeven minutes, the images *a b* and *v w* were vaniſhed; but, examining them again ſoon after, the images were again viſible, and in every reſpect the ſame as they appeared before. A ſhort time after, they diſappeared, and did not appear any more.

Soon after the above appearances, I obſerved a ſhip C, with the hulk below the horizon *x y*, paſſing by the ſame cliffs AB; an inverted image D of which appeared againſt the cliffs, as repreſented in Fig. 6. The ſhip was in motion, and remained at the ſame diſtance on the other ſide of the horizon: I continued my obſervations upon it till it had paſſed the cliffs for a conſiderable diſtance, but there was no change of appearance. The cliffs were illuminated by the ſun, and appeared very diſtinctly; but there was no image above, as in the laſt caſe.

Continuing to obſerve the ſame cliffs AB, Fig. 7, I ſoon after diſcovered two partial elevations, *m*, *n*, of the ſea, by the unuſual refraction; they changed their figures a little, and diſappeared in the place where they firſt appeared, and were equally diſtinct in every part.

About this time, I obſerved a very thick fog coming upon the horizon from the other ſide, rolling upon it with a prodigious velocity; curling as it went along, like volumes of ſmoke ſometimes out of a chimney. This appeared ſeveral times. I conclude, therefore, that there was a conſiderable fog on the other ſide of the horizon.

The laſt phænomenon which I obſerved was that which is repreſented in Fig. 8.; where *x y* repreſents the horizon, *a b* two partial elevations of the ſea, meeting at *c*, and continued to *d*: *e*, another partial elevation of the ſea, of which kind I obſerved ſeveral, ſome of which moved parallel to the horizon with a very great velocity. I conjecture, therefore, that theſe appearances were, in part at leaſt, cauſed by the fog on the other ſide of the horizon. For, though I did not at the ſame time ſee the motion of theſe images and that of the fog, yet, from memory, I judged the motions to be equal; and they were alſo in the ſame direction. A fog which, by producing an unuſual refraction, might form theſe images, would, by its motion, produce a correſponding motion of the images.

I have here deſcribed all the different phænomena which I obſerved from the unuſual refraction, of moſt of which I ſaw a great many inſtances. Every ſhip which I obſerved on the other ſide of the horizon of the ſea, exhibited phænomena of the kind here deſcribed, but not in the ſame degree. Of two ſhips which, in different parts, were equally ſunk below the horizon, the inverted image of one would but juſt begin to appear, whilſt that of the other would repreſent nearly the whole of the ſhip. But this I obſerved, in general, that as the ſhip gradually deſcended below the horizon, more of the image gradually appeared, and it aſcended; and the contrary, when the ſhips were aſcending. Upon the horizon, in different parts, one ſhip would have a complete inverted image; another would have only a partial image; and a third would have no image at all. The images were in general extremely well defined; and frequently appeared as clear and ſharp as the ſhips themſelves, and of the ſame magnitude. Of the ſhips on this ſide of the horizon, no phænomena of this kind appeared. There was no fog upon our coaſt, and the ſhips in the Downs, and the South Foreland, exhibited no uncommon appearances. The uſual refraction at the ſame time was uncommonly great; for the tide was high, and at the very edge of the water I could ſee the cliffs at Calais a very conſiderable height above the horizon; whereas they are frequently not to be ſeen in clear weather from the high lands about the place. The French coaſt alſo appeared both ways, to a much greater diſtance than I ever obſerved it at any other time; particularly towards the eaſt, on which part alſo the unuſual refraction was the ſtrongeſt.

During the remainder of my ſtay at Ramſgate, which was about five weeks, I continued daily to examine all the ſhips in ſight; but I diſcovered no phænomena ſimilar to thoſe which I have here given a deſcription of. The phænomenon of the ſhip obſerved by Mr. Huddart, differed altogether from thoſe above deſcribed, as the inverted image which he obſerved was below the ſhip itſelf. An appearance of this kind I obſerved on Auguſt the 17th, about half an hour after three o'clock in the afternoon, of which Fig. 9. is a repreſentation. The real ſhip is repreſented by A, and the image by B; er, mv, the hulks; st the flag, and wx its image, juſt touching it, with the ſea xy below. Between the two hulks ſome faint dark ſpots and lines appeared, but I could not diſcover what they were the repreſentatives of. The veſſel, at the time of this appearance, was not quite come up to the horizon; and, as it approached it, the image gradually diminiſhed, and totally diſappeared when the ſhip arrived at the horizon.

It remains now, that we inquire into the cauſes which might produce the very extraordinary effects which have been above related. From the phænomena, we are immediately led to the nature of the path of the rays of light to produce them; and we may conceive, that the air may poſſibly be in ſuch a ſtate as will account for the unuſual tract which they muſt have deſcribed. For, let bz (Fig. 10.) be the ſurface of the ſea; ab an object; E the place of the eye; arE, bsE, the progreſs of two rays, by the uſual refraction, from the extreme parts of the object to the eye; to theſe curves draw the tangents Ea', Eb', and $a'\,b'$ will be the image of the object, as uſually formed. Now, if we take the caſe repreſented in Fig. 4, let $a''\,b''$ repreſent the inverted image, and $a'''\,b'''$ the erect image; join a''E, a'''E, and b''E, b'''E, and theſe lines muſt reſpectively be the directions of the rays entering the eye from a and b, in order to produce the images $a''\,b''$ and $a'''\,b'''$; hence, theſe lines muſt be tangents at E, to the curves which are deſcribed by the rays of light; let therefore anE, amE, bvE, bwE, be the curves deſcribed. We have therefore to aſſign a cauſe which may bring rays paſſing above the rays arE, bsE, to the eye at E. Now, if there were no variation of the refractive power of the air, a ray of light paſſing through it would deſcribe a ſtraight line; therefore, the curvature of a ray of light paſſing through the atmoſphere, depends upon the *variation* of the refractive power of the air. If, therefore, we ſuppoſe the air lying above arE, to vary quicker in its refractive power than the air through which arE paſſes, the curvature of a ray proceeding above that of arE, will be greater than the curvature of arE; and upon this principle we may conceive that a ray may deſcribe the curve anE: and, in like manner, if a quicker variation of refractive power ſhould take place above the curve anE, than in that curve, a third ray may deſcribe the curve amE. The ſame may be ſaid for the rays bvE, bwE, diverging from b. The alterations of the refractive power may ariſe, partly from the variations of its denſity, and partly from the variations of its moiſture; and the paſſage of the rays through the boundary of the fog may there ſuffer a very conſiderable refraction; for, from the motion of the fog, and that of the images above mentioned, I have no doubt that the fog was a very conſiderable agent in producing the phænomena. When all the cauſes co-operate, I can eaſily conceive that they may produce the effects which I have deſcribed. If the cauſe ſhould not operate in the tract of air through which the curves anE, bvE paſs, but ſhould operate in the tract through which amE, bwE paſs, an erect image would be viſible, but there would be no inverted image; and, ſhould it operate in the latter caſe, but not in the former, there would be only an inverted image.

As the phænomena are very curious, and extraordinary in their nature, and have not, that I know of, been before obſerved, I have thought proper to lay a deſcription of them, with all the attending circumſtances, before the Royal Society. They appear to be of conſiderable importance, as they lead us to a knowledge of thoſe changes to which the lower parts of the atmoſphere are ſometimes ſubject. If, when theſe phænomena appear, a veſſel, furniſhed with a barometer, thermometer, and hygrometer, below, and alſo at the top of the maſt, were ſent out to paſs below the horizon and return again; and an obſerver at land, having like inſtruments, were to note, at certain intervals, the ſituation and figure of the images, it might throw further light upon this ſubject, and lead to uſeful diſcoveries reſpecting the ſtate of the atmoſphere, from a conjunction of the cauſes which affect theſe inſtruments.

VII. *Caſe of Tetanus cured by Wine. Communicated in a Letter to Dr.* JAMES GREGORY, *Profeſſor of the Practice of Phyſic in the Univerſity of Edinburgh, by Dr.* DAVID HOSACK, *Profeſſor of Botany and Materia Medica in Columbia College*[*].

THE treatment of lock-jaw by the uſe of tonic remedies, has been long ſince ſanctioned by the ſucceſsful practice of Dr. Cochran of Nevis, (ſee Med. Commentaries, Vol. III.) Dr. Wright of Jamaica, (ſee London Med. Obſ. and Inq. Vol. VI.) Dr. Ruſh of Philadelphia, (ſee American Philoſ. Tranſactions,) and Dr. Currie of Liverpool, (ſee Med. Mem. of London, Vol. III.) but the ſame treatment, in other hands, has not been equally ſucceſsful, inſomuch that it is ſtill conſidered as one of thoſe diſeaſes which generally baffle medical preſcription. To what cauſes is this failure to be aſcribed? Not to any defect in the principle upon which thoſe remedies have been preſcribed: the caſes which have been recorded of the ſucceſs of tonic medicines are too numerous to admit a doubt, that the proximate cauſe of lock-jaw conſiſts in an exhauſted ſtate of the ſenſorial power from violent irritation applied to the nervous ſyſtem: ſtimulants and tonics are therefore the remedies which are beſt calculated to reſtore this loſt energy. Their failure I have conſidered to proceed from the complicated and inert manner in which they have uſually been adminiſtered.

In the greater number of caſes which I have either witneſſed in practice, or remember to have ſeen deſcribed, the bark, wine, cold-bath, and, in many inſtances, opium, muſk, and mercury, have been exhibited at the ſame time: I muſt except one caſe, recorded by Dr. Currie, in the Memoirs of the Medical Society of London, in which, the patient rejecting the mixture of bark and wine, the bark was omitted, and the wine was employed alone, which ultimately effected a cure; but in this caſe, opium, mercury, and the cold and warm bath, had been previouſly employed, but to no purpoſe.

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[*] From the *American Medical Repoſitory*, Vol. III. No. 1.

This complicated mode of practice cannot but be prejudicial in any diſeaſe; in lock-jaw it muſt eſpecially prove injurious, by haraſſing the patient, and by offending the ſtomach with the diſcordant and nauſeous mixture of the remedies above mentioned: if, therefore, a practice more ſimple, and, at the ſame time, more efficacious can be deviſed, it is certainly a deſideratum in the treatment of this formidable and fatal diſeaſe.

Having, in a variety of diſeaſes, attended with great exhauſtion of the vital powers, employed wine alone, with ſucceſs, without the uſe of thoſe remedies which are uſually preſcribed in this condition of the body, I long ſince reſolved to give it a trial in lock-jaw.

In January 1798, a merchant of this city, while engaged in opening a box of goods, ſtruck the inſide of his right hand upon a nail; the ſkin was conſiderably torn, but the wound did not appear to extend beneath the integuments. In twenty-four hours his hand became painful, and ſwelled, attended with great heat and redneſs, which ſpread over the wriſt. He immediately applied a poultice of bread and milk to the part affected. In forty-eight hours the pain extended the whole length of his arm, and produced ſome uneaſineſs about his throat, eſpecially in the act of chewing and ſwallowing. He became alarmed, and applied to me for advice. I found him in great pain; but being free from fever, I directed him to have recourſe to wine; to take a large wine-glaſs full every hour until his pain was removed; and, in addition to the uſe of wine, to apply a compreſs, wet with ſpirits, to the wound. When he had taken to the amount of four glaſſes, he felt himſelf very ſenſibly relieved; and by the occaſional uſe of the wine for twenty-four hours, his pains entirely left him; the ſwelling ſubſided, and in a few days the wound was healed, without any unpleaſant appearances.

What would have been the progreſs and termination of this caſe, had it been left to itſelf, is uncertain; but the immediate good effects of the treatment preſcribed, encouraged me to make trial of the ſame remedy in a caſe where the diſeaſe might appear in a more formidable ſhape.

On Tueſday, March 13, 1798, about one o'clock P. M. I was called to viſit a mulatto ſervant woman of John Harrington, Eſq. of this city. I was informed that about an hour before, while engaged in waſhing clothes, ſhe had pricked herſelf with a pin in the wriſt of her right arm. The part at which the pin entered was upon the inſide of the wriſt, immediately over the connection of the radius with the carpus.

The pin was inſtantly removed, and, finding no inconvenience from the accident, ſhe returned to her employment. In a ſhort time ſhe felt a great degree of ſoreneſs in the part which had been injured, with pain ſhooting occaſionally to the arm, ſhoulder, and neck. Theſe ſymptoms, in a few minutes, were ſucceeded by ſtiffneſs about the throat, difficulty of ſwallowing, ſome interruption of her ſpeech, and, at length, a locked ſtate of the jaws, attended with a ſpaſmodic contraction of the muſcles at the back part of the neck, and occaſional ſubſultus tendinum, with ſome coldneſs of her extremities. In this ſituation I found her.

She was naturally of a delicate and irritable habit of body, and had been much ſubject to hyſterical complaints and fits of fainting, which were ſometimes induced by the moſt trifling cauſes. Her irritability of habit was alſo at this time probably increaſed, having but three months before borne a child, which ſhe was then ſuckling.

Although I have been long ſince convinced of the inſufficiency of opium in the cure of this diſeaſe, in the hurry of the moment I gave her about ſixty drops of laudanum in a ſmall quantity of wine. Her jaws being cloſely locked, it was with great difficulty adminiſtered. In a few minutes after ſwallowing the laudanum ſhe ſickened at the ſtomach, and vomited violently, complaining at the ſame time of great pain and diſtreſs at the pit of her ſtomach. The anodyne draught was entirely rejected; but, upon a moment's reflection, I did not regret this circumſtance, as the diſeaſe aſſumed a very decided character, and I had made up my mind to rely upon the effects of wine alone, without the aſſiſtance of any other remedy: accordingly, about two o'clock, I directed a large wine-glaſs full of Madeira wine (the glaſs containing about two ounces,) to be given punctually every hour, and a cup of ſago, or panado, with wine, to be given, from time to time, as her nouriſhment. At this time another phyſician, who had alſo been called upon at the time of the accident, arrived. I related to him what had been done, and the mode of treatment which I directed for the patient. This gentleman having had frequent opportunities of ſeeing this diſeaſe, and having frequently witneſſed the failure of the ordinary mode of treatment, he at once, with great candour, acceded to the plan propoſed, and, in addition to the uſe of wine, propoſed the application of cauſtic to the part which had been wounded. Accordingly the wound was freely pencilled with the lunar cauſtic, and afterwards covered with a poultice of bread and milk, with the view to obtain ſuppuration as ſoon as poſſible.

The wine was adminiſtered with great fidelity, by the mother of the patient, until about five o'clock the next morning. She had ſome ſlight convulſions in the courſe of the afternoon, but they were more of an hyſterical ſort, induced by her great anxiety of mind, than to be aſcribed to the diſeaſe itſelf. Generally ſpeaking, there had been a very manifeſt abatement in all her ſymptoms, and ſhe had paſſed a more comfortable night than could have been expected. At five o'clock on Wedneſday morning, her miſtreſs, alarmed at the quantity of wine ſhe had taken, deſiſted from its further uſe. From this time appearances became more unfavourable, and at eight o'clock her jaws, which had been relaxed during the plentiful uſe of wine, again became ſtiff and cloſed. We ſaw her at nine, and immediately gave her about half a pint of wine, and ordered it to be adminiſtered as before. At one her ſymptoms were greatly changed; we found her ſitting up in bed, eating ſmall portions of roaſted oyſters, which ſhe had called for. At this time her jaws were almoſt in their natural ſtate. She had taken her wine punctually as directed, but experienced no inconvenience from it whatever, although in health ſhe had not been accuſtomed to its uſe. Her pulſes were ſtill ſmall and feeble, without any excitement from the uſe of wine. The heat of body remained at its natural ſtandard, but not at all increaſed. The pain in her head was abated, but without any appearance of ſuppuration. Finding this mode of treatment to agree ſo well with her, we directed it to be continued. We ſaw her again in the evening: her ſymptoms ſtill continued favourable, without the ſmalleſt febrile action from the uſe of wine. Having had no diſcharge from her bowels ſince her illneſs, an injection was adminiſtered; which remedy was afterwards employed, from time to time, in the courſe of her diſeaſe, whenever the ſtate of her bowels required it. The wine was continued through the night: ſhe ſlept, altogether, about three hours in the courſe of the night, and took freely of her panado.

Thurſday morning at nine o'clock her complaints appeared to be, in a great meaſure, ſubdued; inſomuch that we did not think it neceſſary to viſit her again until late in the evening, and directed the wine to be given at longer intervals, and the quantity to be leſſened.

She remained in a very comfortable condition until the afternoon: the pain in her hand returned with violence, extending to her arm and neck as before; her jaws were again cloſed; the rigidity of the muſcles at the back of her neck returned; her mind became greatly agitated; ſhe again complained of diſtreſs at the pit of her ſtomach; ſhe fainted, and had ſeveral ſlight convulſions. Being called at that time, I gave her, with ſome difficulty, about half a pint of wine, and ordered a warm poultice to be immediately boiled. When prepared, I poured upon the ſurface of it half an ounce of laudanum, and applied it to the wound. Her ſymptoms were in a ſhort time allayed: I left her, directing the wine to be continued as before, a large wine-glaſs full every hour.

We ſaw her again at nine in the evening. She remained tranquil: her jaws were leſs firmly cloſed, but the pain in her hand was not altogether removed. Although ſhe had taken the wine punctually as directed, it had not produced the leaſt apparent excitement. Having had no diſcharge from her bowels for the laſt twenty-four hours, an injection was adminiſtered. The anodyne poultice was renewed; and, in addition to this application, we directed her arm to be bathed with laudanum occaſionally through the night.

Friday morning we found ſhe had paſſed a more comfortable night than the laſt; had taken her wine every hour; her jaws were perfectly relaxed; the pain in her hand had greatly abated, and ſhe was enabled to extend her fingers at pleaſure, which ſhe could not do before. Her pulſes and ſkin were natural; her appetite unimpaired; her mind compoſed, without any inconvenience from the wine. We directed her remedies to be all continued as before, fearing leſt any alteration might ſubject her to a return of her complaints.

In the evening we obſerved the wine had exhilarated her ſpirits; ſhe became very talkative; her pulſes became full, and free from all tenſion; her ſkin was ſomewhat heated, and all complaints removed except the wound at the wriſt, which exhibited a healthy appearance, and was entirely free from pain, but without any ſign of ſuppuration.

We directed the wine to be adminiſtered through the night, but in ſmaller quantities and at longer intervals, unleſs her complaints ſhould return and demand a continuance of it as before.

Saturday morning we were informed ſhe had ſlept the greater part of the night, and had taken but a ſmall quantity of wine: her ſymptoms being, in all reſpects, favourable, the wine was diſcontinued, except a ſmall quantity mixed with nouriſhment. A dreſſing of ſimple ointment was applied to the wound. From that time ſhe remained free from any return of her complaints, and has ſince been in perfect health.

Upon calculating the quantity of wine which ſhe had taken, it amounted to three gallons.

VIII. *On*

VIII. *On the Expanſion of Water during Congelation. Communicated in a Letter from* STEPHEN DICKSON, *M.D. to Dr.* MITCHILL[*].

IT is well known that water, in the act of freezing, expands with conſiderable force. By calculations inſtituted on ſome experiments of the Florentine academicians, this force appears to be ſo great, as, in a ſpherule of water only one inch in diameter, to be ſuperior to a reſiſtance of thirteen tons and a half.

Some ingenious experiments on this ſubject were made, a few years ago, in Quebec, during a very cold ſeaſon, by Major Williams. He expoſed to intenſe cold, water incloſed in an iron bomb-ſhell: the fuze was expelled to a conſiderable diſtance, and a cylinder of cryſtalliſed ice was ſhot forth from the aperture, which, by Dr. Hutton's calculation[†], in one inſtance, amounted to upwards of an eighteenth part of its original bulk.

The philoſopher who obſerves that this is not the uniform effort of ſpecific particles mutually receding from one another, ſince the ſpecific gravity of ice is greater than that of water, naturally inquires to what cauſe it is to be attributed. None ſeems to have a greater appearance of veriſimilitude than the ſuggeſtion of Dr. Black; *viz.* that, in the moment of the converſion of water into ice, the latent heat ſet looſe enters into the air contiguous to, or combined with, the water, and expands it with ſuch vehemence as to effect the divarication of the ice into irregular maſſes, and the exploſion of reſiſting bodies. This explanation is countenanced by a minuter attention to the experiments to which it is applied; for we find that ordinary rain-water was employed, and even that the bomb-ſhells were but "nearly filled."

To put the inquiry, however, to a more rigid teſt, I procured two exactly equal and ſimilar cylinders of tin, cloſed at the extremities, except by a ſmall pipe in each iſſuing from the upper ſurface. One of theſe I filled with pure but ordinary rain-water; the other with water which I had carefully diſtilled. The apertures of both were cloſed alike by well fitted plugs of wood, which I had previouſly boiled in oil. I expoſed them to the open air, on the ſurface of the ſnow, in Quebec, the night of the 5th of January 1799, when the thermometer ſtood at 28 degrees below zero of Fahrenheit's ſcale. About twenty minutes afterwards, I found both the cylinders inclining to the horizon at an angle of about thirteen degrees; being ſo far rent from their baſes, which yet reſted on the ſnow. The intermediate ſpace in each was filled with rude maſſes of ice, which, on being examined with the microſcope, (in a room,) appeared to be compoſed of cryſtals, chiefly in the form of parallelopipeds and truncated pyramids. The whole of the appearances, in both caſes, were exactly ſimilar. I afterwards repeated theſe experiments, without any variety in the reſult.

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[*] From the *American Medical Repoſitory*, Vol. III. No. 1.

[†] Tranſactions of the Royal Society of Edinburgh, Vol. II.

From hence we perceive evidently that Dr. Black's ſolution is inadequate to account for theſe remarkable phenomena. At the ſame time I think it impoſſible not to admit the original principles upon which his ingenious rationale reſts. It is impoſſible that water can be converted into ice without the evolution of one-tenth of the whole quantity of its ſpecific fire, which is equal to 146 degrees of Fahrenheit's thermometer. Now, we know that water can be converted into vapour, under particular circumſtances, by much inferior temperature.—*Quære.* Muſt not part of the water have been converted into vapour in the very proceſs of congelation; and may not this cauſe be adequate to the production of the moſt violent expanſive force of froſt?

Quebec, January 24, 1799.

IX. *The*

IX. *The Theory of Exploſions. Communicated in a Letter from Mr.* Blanchet *to Dr.* Mitchill, *Profeſſor of Chemiſtry, Natural Hiſtory, and Agriculture, in Columbia College, in the State of New-York.*

THE cauſe of exploſions, in general, which, at firſt view, appears to be ſimple and eaſy to comprehend, is, nevertheleſs, beſet with many difficulties; and it is with much reluctance that I have ventured to diſcuſs this ſubject.

In order to avoid a tireſome recital, I ſhall decline to ſtate the opinions of philoſophers upon this ſubject. My deſign is to explain the immediate cauſe of certain phenomena, which ſeem hitherto to have eluded their reſearch. The phenomena, which ſeem to be exceptions from general principles, becauſe they are preſented under an irregular and peculiar form, are included in the following queſtions:—What is the immediate cauſe of the burſting of veſſels containing water in the ſtate of congelation? In what manner does lightning rend and ſhiver a tree into ſo many pieces? Whence ariſe volcanic exploſions and earthquakes? Theſe inquiries form the ſubject of my letter.

As to the immediate cauſe of the burſting of veſſels filled with congealing water, the celebrated Dr. Black, according to the obſervation of Dr. Dickſon, (ſee Medical Repoſitory, Vol. III. p. 35.) ſeems, in ſome degree, to have underſtood it, when he ſuppoſes, "that in the moment of the converſion of water into ice, the latent heat of the water, then ſet at liberty, enters into the air which is contiguous to its ſurface, or combined with the water, and dilates it with ſuch force that it produces a ſeparation of the ice into an irregular form, and a burſting of the containing and reſiſting veſſels." But this explanation, however ingenious, is not entirely ſatisfactory. Beſides the caloric extricated from freezing water, which may dilate the air combined with it, there is another agent which eſcaped Dr. Black's attention; that is, the ſudden detachment of atmoſpheric air which is found lodged in the interſtices of the particles of the water.

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* From the *American Medical Repoſitory*, Vol. III. No. 3.

The moſt ſimple principles of natural knowledge teach us that atmoſpheric air cannot paſs from its permanently aëriform ſtate to that of fluid or ſolid, by any degree of cold with which we are acquainted. Admitting this to be true, there is no longer any ground of ſurpriſe in ſeeing the burſting of bodies which reſiſt the force of freezing water. The air, a fluid of permanent elaſticity, being compreſſed, and forced to give way to the particles of water, which cold, or the attractive power of the atoms of bodies, draws violently together, it follows that the ice, into which the water is converted, muſt break aſunder, in order to afford a paſſage to this aëriform ſubſtance, which cold can neither condenſe nor make ſolid, and muſt rend and ſhiver, with a cracking noiſe, the veſſels no longer able to hold it.

This theory is rendered probable, and ſtrengthened, by a fact which I have obſerved myſelf, *viz.* that the fats, ſuch as hog's fat, &c. ſettle conſiderably down in the veſſels in which they are condenſed. I have alſo noticed cavities in the interior parts of ſuch maſſes of fat. Now, this ſhrinking of volume, and theſe cavities in fat, undoubtedly, can only ariſe from the extrication of atmoſpheric air in the proceſs of condenſation. Independently of this fact, which every body is ready to atteſt, there is not a Canadian peaſant who is ignorant that the cavities found in a maſs of ice were formed by air, and contain it. Theſe facts, therefore, prove that, in the congelation of water, the atmoſpheric air which exiſts in the interſtices of the particles of water is forced to detach, or ſet itſelf at liberty, as ſoon as cold converts them into ice.

On this principle it is eaſy to explain the rending and ſplitting of trees, with a cracking noiſe, during the froſts of winter; the burſting of ſtones in the moment of the converſion of the water contained in them into ice; and the breaking, as well as the loſs of the primitive perpendicularity of walls which had been built during the autumnal ſeaſon.

According to the Academy *Del Cimento*, it appears to be demonſtratively true, that frozen water occupies more bulk than fluid water. This opinion ſeems to claſh with all fact and analogy: for we conſider it as aſcertained, that the ſpecific gravity of ice is greater than that of water; that is to ſay, that a cubic foot of frozen water contains more watery particles than a cubic foot of fluid water, ſince, in the former caſe, the particles are undeniably more cloſely approximated and compreſſed than in the latter. Now, is it poſſible that frozen water, whoſe particles have been condenſed, occupies more ſpace than water in the ſtate of fluidity? I have often ſeen water freezing in open veſſels, ſuch as the copper veſſels of kitchens, &c., and I have always remarked, towards the middle of the veſſel, a riſing, divided by chinks, which doubtleſs had been formed by the eſcape of atmoſpheric air during the congelation of the water. It is this phenomenon which has probably led to a wrong concluſion. Beſides, the hog's fat, which, like water, becomes ſolid by cold, or by the abſence of caloric, is diminiſhed in bulk by condenſation; and, if the air which eſcapes in this proceſs produce no burſting, it is becauſe the fat is always ſet to cool in open veſſels. Upon what ground, then, could an exception be made to the general law? But I now come to explain the effect produced by lightning when it ſtrikes a tree, &c.

No perſon, ſo far as I know, ſeems yet to have accounted for the ſhattering of trees by the ſtroke of lightning. A moment's reflection, however, ſuffices to diſcover and explain the cauſe. As it is only by a careful analyſis of facts that we can deduce juſt concluſions, I proceed to examine what happens in the moment of the lightning ſtriking a tree, and it will be ſeen whether this ladder will enable us to reach to the cauſe of this phenomenon.

Since the beautiful experiments of Lavoiſier, of Fourcroy, &c. have demonſtrated that caloric is the cauſe of the repulſive power of the atoms of bodies, there is no doubt that the more the atoms of a body are removed from one another, the greater is the capacity of ſuch a body to contain caloric, or the more caloric it contains. It is alſo inconteſtably proved, that the bodies which are conſtantly in the ſtate of gas, in the common temperature of the atmoſphere, contain a great quantity of caloric, and in proportion to their ſpecific capacity for holding it. So likewiſe the electric fluid, or what might, with more propriety, be called the electric gas, muſt contain a prodigious quantity of caloric, ſince this element always exiſts in the ſtate of gas, and the more this gas is accumulated, the more the amount of ſpecific heat muſt alſo be augmented.

This being premiſed, whenever the lightning (which is only an accumulation of electric gas, diſcharged from one cloud to paſs to another containing a leſs quantity) happens, in its courſe, to ſtrike a tree, &c. the tree becomes ſo ſuddenly overcharged with caloric, that the air and other conſtituent parts of it, which are diſpoſed rapidly to combine with heat, undergo expanſion, and thereby ſhiver the tree, in order to force a paſſage, and to ſet themſelves at liberty. I cannot compare this phenomenon to any thing better than to the diſcharge of a cannon or of a muſket; for every body knows that the diſcharge of a cannon is owing to the ſudden expanſion of the conſtituent parts of the powder by the introduction of caloric.

In like manner, I have every reaſon to believe that the violent rending of a tree, by lightning or the electric gas, is owing to a ſimilar cauſe; that is, a ſudden accumulation of caloric, which expands, with violence, the air, &c. lodged in the interſtices of that body. This opinion will be readily embraced, if it be conſidered that the electric gas melts the metals. If, indeed, the metals be ſuddenly melted by this gas, or rather by the caloric which it contains, is it not thereby demonſtrated that ſuch a degree of heat is ſufficient immediately to ſet fire to all combuſtible* bodies, and ſo far to expand the air contained in the tree as to cauſe the rending of its parts?

If animals which are killed by a ſtroke of lightning, or by the accumulation of the electric gas, receive no apparent injury, ſuch as laceration of their parts, as in the caſe of the tree, their death muſt be owing to a decompoſition produced in their bodies. This opinion, which has never been attended to, ſhall be fully developed and diſcuſſed in another work. I proceed, therefore, to the third part of my ſubject;

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* The experiment of Meſſrs. Lavoiſier and Meunier, in which they cauſed an electric ſpark to paſs through a balloon which contained oxygen and hydrogen, to produce combuſtion in order to form water, proves how large a quantity of caloric is contained in the electric gas.

to wit, to inquire concerning the immediate cauſe of the eruption of volcanoes and of earthquakes.

Whatever may be the agent which kindles and inflames volcanic ſubſtances, whether it be the electric gas which communicates its fire to thoſe moſt inflammable ſubſtances, ſuch as ſulphur, &c. or whether it be the ſubterranean heat, which, in making its eſcape, ſets fire to thoſe ſubſtances, ſtill it is certain that atmoſpheric air muſt be found in great quantities in the interior of volcanoes. For, ſince the ever memorable diſcoveries of Lavoiſier, of Prieſtley, of Scheele, and of Ingenhouz, on combuſtion, it is admitted as a demonſtrable truth, that the burning of combuſtible ſubſtances is owing to oxygen; that is to ſay, it is fixed and abſorbed by the body in combuſtion, while the caloric and the light, which are combined with it, eſcape, in order to form the flame, the light, and the heat. In this manner atmoſpheric air is one of the eſſential agents in ſupporting the fire of a volcano.

Theſe things being premiſed, as ſoon as a volcano is kindled and ſet on fire, the atmoſpheric air which feeds it, as well as the baſes of ſulphur, of water, &c. which are alſo converted to the ſtate of gaſes by the evolution or accumulation of heat, are expanded, and break out with a violence and impetuoſity not to be reſtrained by the force of oppoſing bodies, and thereby give birth to earthquakes, &c.

This theory is remarkably countenanced by the common and uniform ſeries of occurrences which follows the eruption of a volcano. In the firſt place, as ſoon as it is kindled, there appears a thick ſmoke; ſoon after, when the air, &c. become expanded, ſtones of enormous bulk are thrown out to great diſtances into the aërial regions, accompanied with earthquake, and agitation of all the ſurrounding country. As ſoon as the ſuperfluous air is diſcharged and ſet free, there ſucceeds a bright flame, which ſeems to reſtore ſerenity to the abyſs, which again, after a ſhort time, vomits forth a ſhower of fiery and deſtructive matters.

To recapitulate: 1. The burſting of veſſels in the freezing of water is owing to the eſcape of atmoſpherical air, which alſo ſeparates the ice into irregular maſſes. Nothing is more improbable than the expanſion or increaſed repellency of the particles of water in the proceſs of congelation. This gives an anſwer to the queſtion of Dr. Dickſon, (ſee Med. Repoſ. Vol. III. p. 34.) 2. The ſhivering of a tree by lightning is cauſed by heat expanding the air, &c. which it contains. 3. The eruption of volcanoes and earthquakes is likewiſe produced by the expanſion of air. In a word, every exploſion is owing to the ſudden extrication of ſome matter incapable of confinement, or to the expanſion of air in a reſiſting body.

I am, with much eſteem and conſideration, Sir,
Your moſt humble ſervant,
F. BLANCHET.

Eſſex County (*N. J.*), *Aug.* 15, 1799.

X. *Letter from Dr.* VAN MONS, *of Bruſſels, to* J. C. DELAMETHERIE, *on the conſtituent Principles of fixed Alkalies, &c.*[*]

CRAANER, an apothecary at Amſterdam, has juſt made an experiment from which it would appear that fixed alkalies contain carbon. He introduced potaſh or ſoda, beſprinkled with water, under a bell filled with oxygen gas. The gas was abſorbed, and the alkali became efferveſcent. Beſides this, an alkali which has been treated ſeveral times with this gas, or which has been ſuffered at ſeveral times to become carbonated in pure air or atmoſpheric air, is found exhauſted of carbon, and can be carbonated no longer. This fact may explain why alkalies are ſo ſpeedily converted into alkaline carbonates even at elevations in the atmoſphere which the carbonic acid never reaches.

I long ago publiſhed that potaſh is formed during the combuſtion of hydro-carbon, and during the fermentation of the juice of grapes, by the intervention of the azot of the atmoſphere When theſe operations take place in pure oxygen, you obtain neither potaſh nor acidulous tartrite of that alkali, and the wine becomes ſo acid that it cannot be drunk. It is poſſible that fixed alkali is an azoto-carbon, or carbon of azot.

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[*] From the *Journal de Phyſique* for Floreal, an. 8.

I, however, formerly made an experiment which is contrary to that of Craaner; I treated in a retort, at a red heat, with iron, a mixture of cauſtic potaſh and red oxyd of mercury, and obtained only oxygen gas, nitric acid, and water. The alkali was totally diſperſed. Another experiment, which I have juſt made, is equally unfavourable to it: I pounded oxygenated muriat of potaſh with cryſtalliſed cauſtic potaſh, and poured the mixture, which had become liquid, into a bottle: I corked the bottle very cloſely, and at the end of three days poured over it muriatic acid, but I obſerved no effervеſcence. As carbon is not combuſtible in the air but at a red heat, if this principle enters into the compoſition of alkalies, it muſt be united in them with hydrogen, unleſs the azot ſerves as the medium of its union with the oxygen.

Brugnatelli has ſeparated butter from cream without the aid of oxygen. He poured into one part of cream four parts of warm water, and made the mixture paſs through a filter of cloth into a cloſe veſſel, and in which were extended ſeveral other filters of the like kind formed of coarſe cloth. At the end of twenty-four hours the ſerous part was found imbibed by the cloth, and the butter completely ſeparated.

Rouppe and Bicker have deſcribed, in the firſt volume of the New Memoirs of the Society of Rotterdam, a new diaſtatometer, which ſeems to anſwer the different purpoſes of ſuch inſtruments. Mention will be made of it in the Annales.

A ſpirit made from the refuſe of the beet-root, and conſidered as ſuperior to French brandy, has been already ſold at Berlin. The houſe of Claude has alſo diſtributed ſamples of arrack made from the ſame ſubſtance, which has exactly the taſte and ſtrength of foreign arrack.

The memoir of Crell on the radical of the boracic acid has been publiſhed ſome time. He treated that acid with oxygenated muriatic acid, and obtained carbon.

Trommſdorff has diſcovered a new earth, which he calls *aguſtine*, from its property of forming, with acids, ſalts deſtitute of taſte.

XI. *Curſory View of ſome of the late Diſcoveries in Science.*

[Continued from Page 314 of the laſt Volume.]

CRYSTALLOGRAPHY.

FOURCROY, in treating urine, obſerved a phenomenon of great importance to cryſtallography. It is known that cubes of ſal-ammoniac and octaedra of marine ſalt have been found in urine. But whence happens it that marine ſalt cryſtalliſes in octaedra, and not in cubes, and that ſal-ammoniac cryſtalliſes in cubes, and not in octaedra? Fourcroy is of opinion that theſe forms are owing to the preſence of *urée*. This he proves by the following experiment:

Marine ſalt, or muriat of ſoda, exceedingly pure and in perfect cubes, having nothing in common with human urine, to which it never belonged, ſince it aroſe from ſpring-water evaporated, was diſſolved, with an equal part of cryſtalliſed *urée*, in five times its weight of diſtilled water. The ſolution was put into a porcelain capſule, which was covered with paper to prevent the acceſs of foreign bodies, and was left to ſpontaneous evaporation. In a few weeks there were formed octaedral cryſtals exceedingly regular, and of a reddiſh brown colour.

Sal-ammoniac, or muriat of ammonia, treated in the ſame manner, cryſtalliſed in cubes, though it cryſtalliſes in general in octaedra.

"It is a fact then, fully proved," ſays he, "that *urée*, diſſolved in the ſame water as the two ſalts above mentioned, modifies and reverſes their natural form by combining with each of them, and by penetrating the laminæ of their cryſtals. To it, therefore, is due the octaedral form aſſumed by marine ſalt, with which human urine has been ſaturated."

Haüy has deſcribed various cryſtals; ſuch as thoſe of cinnabar or ſulphurated mercury, of ſulphat of ſtrontian, and of the arragonite or calcareous ſpar cryſtalliſed in hexagonal priſms, which is found in Arragon.

VOLCANOES.

Kirwan has publiſhed an excellent memoir on the primitive ſtate of the globe, and the cataſtrophe which ſucceeded it.

He

He is of opinion with me *, that the globe was formed by cryſtalliſation. In ſpeaking of mountains, he ſays, that volcanic mountains, ſuch as Veſuvius, Ætna, &c. exiſted anterior to the eruption of volcanoes, and that ſubterranean fires were kindled up in their bowels.

G. A. Deluc has attacked this latter part of the opinion of Kirwan. He thinks that the foci of ſubterranean fires are at a great depth, and that volcanic mountains, ſuch as Veſuvius, Ætna, the Lipari iſlands, &c. were formed chiefly from matters thrown up by volcanoes and accumulated in the neighbourhood.

Breiſlak has given a phyſical topography of Campania. He firſt diſcovered there, in 1793, an extinguiſhed volcano, which ſeems to have been much more conſiderable than Veſuvius, and to approach near to Ætna. It is called Rocca-Montſina. He ſuppoſes with Gioeni, that the firſt origin of Veſuvius was at the bottom of the ſea. He obſerved a ſource of petroleum at the bottom of the ſea oppoſite to Mount Veſuvius, and at the diſtance of ſomewhat leſs than a mile from the ſhore. When drops of this ſubſtance riſe to the ſurface of the water, they form on it ſpots perfectly round, of from three to four inches in diameter, and of a yellowiſh-brown colour. A ſpring of petroleum near Veſuvius might afford ſome aſſiſtance to a framer of ſyſtems. By combining this phenomenon with other ſprings of petroleum in the neighbourhood of the Appenines, and with the foſſil coal of Benevento and Giſone, which nothing prevents us from ſuppoſing to be extended below the earth, we may figure to ourſelves under Veſuvius an immenſe reſervoir of bitumen, kindled by an electric ſhock, or by ſome other unknown cauſe.

The combuſtion will continue as long as the maſs of the reſervoir is not conſumed, and may be renewed whenever a new cauſe of inflammation acts again upon a new quantity of bitumen.

France at preſent is ſubject to earthquakes, which, if we are to judge by the conſiderable number of extinguiſhed volcanoes found there, muſt formerly have been very common.

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* This is extracted from the *Journal de Phyſique*, of which Delametherie is editor.

In the month of January laſt year, a ſlight ſhock of an earthquake was experienced in the whole of the weſt of France, from Rouen to Bourdeaux. The ſame diſtrict experienced alſo a violent agitation in 1755, at the time of the earthquake which deſtroyed Liſbon. No extinguiſhed volcanoes, however, are known in that diſtrict except in the neighbourhood of Treguier.

On the 29th of Prairial there was a terrible earthquake at Acapulco: whole diſtricts were convulſed, and the port was filled up.

Salmon has given a beautiful memoir on the origin of volcanic baſaltes. Some, among whom are the greater part of the French naturaliſts, have conſidered them as the production of fuſion by fire. Others, ſuch as the celebrated Werner, are of opinion that baſaltes has been depoſited by water. Salmon undertakes to reconcile the two hypotheſes, and aſſerts, that they have been produced by an aquoſo-igneous liquefaction. Water reduced to vapour, for example, in Papin's digeſter, acquires a great degree of heat, capable of fuſing various ſubſtances which require a ſtrong heat to be reduced to that ſtate. All volcanic vapours and all ſpiracles contain a great quantity of water.

He is of opinion that ſeveral ſubſtances contained in baſaltes, ſuch as feld-ſpar, augite, hornblende, zeolithes, and mica, have been encloſed in it accidentally while it was liquid. But he thinks that there are ſeveral others, ſuch as leucites, which have been fuſed with the baſaltes, which have afterwards cryſtalliſed apart, and been ſeparated from the maſs by different laws of affinity.

Buch entertains the ſame opinion. He has no doubt that leucite has cryſtalliſed in the maſs of the baſaltes at the moment of its liquidity. The portions of hornblende or of baſaltes, found in the centre of ſeveral cryſtals of leucite, ſeem to theſe naturaliſts a convincing proof of their opinion.

[To be continued.]

NEW PUBLICATIONS.

An Eſſay on the Analyſis of Mineral Waters. By RICHARD KIRWAN, *Eſq. F.R.S. &c.*

IN this excellent treatiſe Mr. Kirwan enumerates the different mineraliſing ſubſtances which have been found in mineral waters; explains by what teſts the preſence of thoſe mineraliſers, whether ſimple or in a ſtate of combination, is to be detected in the waters in which they exiſt; and treats, with eminent ſkill and accuracy, of the methods of proceſs by which the teſts are to be the moſt ſucceſsfully employed in the analyſis of the waters. Tables are ſubjoined of the quantities of real acid in mineral acids of different denſities; of the quantities of acid abſorbed by different baſes; of the quantity of each baſis abſorbed by each acid; of the proportion of ingredients in neutral ſalts; of the length of a column of common air at different barometrical heights and different temperatures. Genius, patience, and accuracy of inveſtigation, and an extenſive acquaintance with the labours of other chemiſts at home and abroad, are eminently conſpicuous in this uſeful work. It has likewiſe the merit of being better written, as a literary compoſition, than moſt Engliſh books upon chemical or phyſical ſcience that have fallen into our hands.

The Clinical Guide; or, A conciſe View of the leading Facts in the Nature and Treatment of Diſeaſes. By WILLIAM NISBET, *M.D. &c.* 3 Vols. 12mo. Johnſon, Saint Paul's Church-yard.

This comprehenſive and well written work, the production of Dr. Niſbet, of Pickering-place, St. James's-ſtreet, includes, in theſe three volumes, a view of the preſent practice of medicine, ſurgery, and midwifery. A fourth volume, on the diſeaſes of children, is, we believe, to be ſoon added, to complete the author's deſign. A work, uniting, more correctly, a larger proportion of rational theory with ſound practical information concerning the ſubjects of which it treats, will not be eaſily named. We need ſcarcely recommend it; for the public already know, and highly approve its merits.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

FRENCH NATIONAL INSTITUTE.

C. COULOMB read in the Inſtitute on the 26th of May, a curious memoir on the reſiſtance of fluids when the movement is exceedingly ſlow, and he has found that it is then merely as the velocity, inſtead of being as the ſquare of the velocity. For his experiments he employed the force of torſion, of which he firſt formed an idea. He complains that Mr. Cavendiſh, in the Philoſophical Tranſactions for laſt year, aſcribes this invention to an Engliſhman who never publiſhed any thing on the ſubject.

LALANDE.

ASTRONOMY.

The obſervations of the Arabs in the tenth century are extremely valuable. We were acquainted with only three, when I diſcovered among the manuſcripts of my former maſter, Joſeph Deliſle, an Arabic copy of a part of the work of Ibn Iunis, where there are a great many. But the original was at Leyden; and we have long ſolicited the Batavian government to intruſt us with it. At length, on the 26th of May, our ambaſſador brought to the Inſtitute this valuable manuſcript, containing 400 pages in quarto ſmall characters; and we hope that not only a tranſlation of it will be printed, but that part alſo of the Arabic original which is intereſting to aſtronomy. C. Chauſſin has already tranſlated that which I procured for him.

LALANDE.

THE CONNECTICUT ACADEMY OF ARTS AND SCIENCES.

A number of literary gentlemen in Connecticut have lately aſſociated for the purpoſe of encouraging philoſophical reſearches, and, particularly, for developing the natural hiſtory of that State. The aſſociation has aſſumed the title of *The Connecticut Academy of Arts and Sciences*. The general meeting of the Academy, for the elction of officers, is to be held, annually, at New-Haven, on the fourth Tueſday of October; and the other meetings are to be on the fourth Tueſdays of December, February, April, June, and Auguſt, at New-Haven.

CHEMISTRY.

The following articles of chemical news were communicated to Samuel L. Mitchill, M. D. Profeſſor of Chemiſtry in Columbia College, New-York, by James Woodhouſe, M. D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, in a letter dated Auguſt 22, 1799:

1. *Of the non-Action of the Nitric Acid on Silver, Copper, and Tin.*

Having occaſion to make a ſolution of ſilver in the nitric acid, ſeveral thin pieces of ſilver were digeſted forty-eight hours, in a ſmall quantity of the moſt pure and concentrated acid, prepared by diſtilling ſtrong ſulphuric acid on nitre, from which the water of cryſtalliſation had been thrown off by means of heat, and the metal was not diſſolved. The temperature of the air varied between 75 and 90 degrees of Fahrenheit's thermometer.

This phenomenon was contrary to what ought to have taken place, according to the chemiſts of all nations, who declare that the nitric acid diſſolves ſilver with the utmoſt rapidity.

Suppoſing that the non-action of the acid was owing to the metal being in ſmall maſſes, the filings of ſilver were tried, but no ſolution took place in the ſpace of two days. Having then added a ſmall quantity of water to the acid, the ſilver was diſſolved in a few minutes.

Repeating this experiment upon copper, the ſame effect happened.

Nitric acid was poured upon copper, and no action was produced; but, uppon the addition of water, ſolution immediately commenced, and oxygenous and nitrous air was diſcharged; the latter holding a portion of the copper in ſolution, as appeared by immerſing a lighted taper in the nitrous acid gas, when it burned with an enlarged vivid and blue flame. The flame of the taper was frequently blown out, and rekindled by dipping it into the air.

Some concentrated nitric acid was alſo poured upon tin foil, when it remained in a quieſcent ſtate for the ſpace of one week; but, upon the addition of water, the whole was inſtantaneouſly converted into a white oxyd, with the production of a high degree of heat.

The errors of chemiſts, in regard to the action of nitric acid upon tin, will be ſeen more clearly by extracting what has been ſaid upon the ſubject.

Chaptal tells us the nitric acid devours tin, that the decompoſition is ſpeedy, and that the metal is inſtantly precipitated in the form of a white oxyd. The ſame author ſays, Mr. Baumé even pretends that the nitric acid does not diſſolve tin; but Kunckel and the famous Rouelle have maintained the contrary.

Fourcroy declares that tin decompoſes nitric acid, even in the cold, with amazing rapidity, and that this is one of the moſt aſtoniſhingly rapid ſolutions in all chemiſtry.

From what has been ſaid, it appears that Mr. Baumé is right, and that Fourcroy, Chaptal, Rouelle, and Kunckel, uſed an acid diluted with water.

In what manner does water act in theſe experiments?

Dr. Prieſtley ſuppoſes that no air can be produced without water, and that it is neceſſary to the conſtitution of every kind of air: but this throws little light upon the ſubject, and it does not account for the manner in which water acts in promoting the ſolution of ſilver, copper, and tin, in the nitric acid; and nitrous air may be obtained from zinc and biſmuth by the acid, however concentrated.

It may be ſuppoſed that the water merely produces heat by uniting with the acid, and ſo diſſolves the metals; but this is not the caſe; for, if the acid is diluted with water, and ſtands until it is cool, it will ſpeedily diſſolve them.

It is a common thing with the teachers of chemiſtry to fold up a portion of the dry nitrat of copper in tin foil, and to let it remain for ſome time in contact with the tin, to ſhow that it will not act upon the metal in a dry ſtate. The tin foil is then unfolded, and a little water is added to the nitrat of copper, and it is again encloſed in the tin; when a violent action enſues, accompanied with ſparks of fire, and a diſcharge of nitrous air.

The intention of this experiment is to ſhow that bodies do not act upon each other in a dry ſtate—*corpora non agunt niſi ſoluta.* But from the experiments which have been related, of the non-action of the nitric acid on tin, the explanation of what takes place muſt be ſought for in the action of the water on the nitric acid of the nitrat of copper.

Some writers have taken notice of the production of ammoniac, when nitrous acid is added to copper and tin. As the concentrated acid has no action on theſe metals, the ammoniac muſt be produced by the hydrogen of the water uniting with the azot of the nitric acid, while its oxygen, and that of the water, unites to the tin and copper, and converts them into oxyds.

Having related theſe facts, the language of chemiſts in future ought to be—The nitric acid has no action on ſilver, copper, and tin; but, if water be added to the acid, ſolution ſpeedily takes place.

Dr. Hope has taken notice of the non-action of the nitric acid on ſtrontian earth; and Mr. Leonhardi tells us, that it quickly deſtroys wool and ſilk, but that linen may remain immerſed in a bottle of the ſtrong acid a whole day without injury.

2. *Of the Difference in the Quantity of Ammoniac obtained from Bones, by diſtilling with and without a Lute.*

A quantity of the bones of horſes and cows were placed in a diſtilling apparatus, formed of iron, which communicated with the worm of a refrigeratory, to which a large glaſs receiver was annexed. Upon applying a high degree of heat, three ounces of volatile alkaline ſpirit, impregnated with the black animal oil of Dippel, were obtained in three hours. The receiver was cloſely luted to the worm, and the air in it was perfectly tranſparent. Upon taking away a part of the lute, in ſuch a manner as to permit the air of the atmoſphere to enter the receiver, it became immediately filled with a thick brown yellow cloud of ſmoke.

Having made a variety of comparative experiments, to determine the difference in the quantity of the product, by diſtilling with and without the lute, it was found that five times as much of the volatile alkaline ſpirit could be obtained by carrying on the diſtillation without the lute, as could be procured, in the ſame ſpace of time, with the application of the lute.

Lavoiſier ſuppoſes, that when ammoniac is obtained from animal ſubſtances, the hydrogen and the azot of theſe bodies unite together, and form the volatile alkali; but it appears, from what has been ſaid, that the azotic air of the atmoſphere enters into the worm of the refrigeratory, joins the hydrogen of the bones, and ſo forms the ammoniac.

Manufacturers of the volatile ſpirit of ſal ammoniac may take ſome valuable hints from theſe experiments.

3. *Of Putrid Urine expoſed to the Froſt.*

A quart of the moſt putrid urine, and of as yellow a colour as gamboge, was expoſed two nights to intenſe cold, when it became perfectly ſweet, and was as colourleſs as rock water.

May not this wonderful change be attributed to the agency of the oxygen gas of the cold atmoſpheric air?

The acid of citrons not only neutraliſes the volatile alkali of putrid ſubſtances, but completely deſtroys the nauſeous ſmell which exiſts independent of the ammoniac. The ſulphuric and muriatic acids have no ſuch effect. Does the oxygen of the citric acid act here likewiſe? Lowitz, a Ruſſian chemiſt, ſuppoſes that charcoal neutraliſes the putrid effluvia of animal bodies; but, in my opinion, it acts mechanically, in preventing the putrid particles of matter from flying into the air.

4. *Of*

4. *Of Starch prepared from the Fruit of the Æſculus Pavia, or Horſe Cheſtnut.*

In the 29th number of the Repertory of Arts, there is an account of a patent, obtained by Lord William Murray, for making ſtarch from the fruit of the æſculus hippocaſtanum. A writer in the London Monthly Magazine for 1798 ſays, he has repeatedly, and in various ways, endeavoured to make ſtarch of the fruit, but always unſucceſsfully; for it turns to a yellow colour.

The fruit of our æſculus pavia is much larger than that of the æſculus hippocaſtanum, and is of a white colour: that of the hippocaſtanum is yellow.

A ſingle nut, dried, weighed half an ounce and twenty-five grains, and yielded forty-four grains of fine ſtarch.

I prepared half a pound of this ſtarch from the nuts of the æſculus pavia, and have kept it two years, and the white colour is no way impaired. It is ſuperior to the fineſt Poland ſtarch, and has been uſed, by ſeveral ladies, to ſtarch various articles of dreſs, without imparting any yellow colour to them.

The method of preparing it is, to take off the ſhells from the nuts with a knife; grate them in a veſſel of water which will hold the fine particles of the ſtarch ſuſpended; when they are to be decanted into another veſſel, which muſt remain at reſt until the ſtarch ſubſides to the bottom. The water is then to be poured off, and freſh is to be added, and the ſtarch is to be well ſtirred about in it; when it muſt be again permitted to ſubſide. The water is then to be thrown away, and the ſtarch is to be dried in the ſun.

The water of the firſt waſhing holds a poiſonous matter in ſolution, which, when evaporated to the conſiſtence of an extract, and mixed with dough, will intoxicate and ſwell the bellies of ſmall fiſhes.

5. *Of the Compoſition of Flores Martiales and Ens Veneris.*

The French chemiſts ſay that the flores martiales are compoſed of ſal ammoniac, coloured by an oxyd of iron; and that ens veneris is ſal ammoniac, coloured by an oxyd of copper: but I can eaſily prove that the one conſiſts of ſal ammoniac and the muriat of iron, and the other of ſal ammoniac and the muriat of copper.

1ſt, When iron or copper is added to ſal ammoniac, the ſalt is decompoſed, the iron or copper unites to the muriatic acid and forms a muriat of iron or copper, which, when expoſed to heat in cloſe veſſels, ſublimes with the ſal ammoniac, while the volatile alkali is ſet at liberty.

2dly, Flores martiales, or ens veneris, may be made by ſubliming ſal ammoniac with the muriat of iron or copper, prepared by adding the muriatic acid to theſe metals.

3dly, When flores martiales, or ens veneris, are diſſolved in water, and ſuffered to cryſtalliſe, the muriat of iron and copper will be obtained ſeparate from the ſal ammoniac.

6. *Of Aromatic Oils, obtained from the Pellicle which envelops the Seeds of the Laurus Saſſafras and Laurus Benzoin.*

The method of obtaining theſe oils is, to boil the pellicle which ſurrounds the ſeeds of the ſaſſafras and benjamin-tree in water; when they float upon its ſurface, from which they may be ſkimmed with a ſpoon.

That of the ſaſſafras differs materially from the oil obtained from the bark of the root of this tree. Its aroma is different; it is much lighter, and it congeals in a higher degree of heat.

The oil of the benzoin-tree is a delightful aromatic, is very inflammable, and might be uſed as a ſpice in food, and in all thoſe diſeaſes in which the aromatic oils are uſeful. It has been tried with ſucceſs, as an external application, in a ſevere caſe of chronic rheumatiſm. One half pound of the pellicle of the ſeeds will yield ſeveral ounce meaſures of oil.

7. *Of the Eudiometer.*

The eudiometer is an uſeleſs inſtrument in aſcertaining the purity of atmoſpheric air.

1ſt, Becauſe nitrous air can never be obtained of the ſame degree of ſtrength.

2dly, When one meaſure of nitrous air is added to one meaſure of atmoſpheric air, the abſorption will be great or ſmall, according to the time the air remains over the water, or is agitated in it.

Having made ſeven trials with this inſtrument, with the ſame atmoſpheric air, I obtained a diminution, at the firſt experiment, of,

	0	60
2d,	0	56
3d,	0	95
4th,	0	87
5th,	0	90
6th,	0	93
7th,	0	100

From this it is evident that Dr. Davidſon was deceived in ſuppoſing that the air of Martinique was much purer than the air of Europe, and that the error lay in his inſtrument. The nitrous air which I uſed was procured from nitric acid diluted with water and copper.

8. *Of the Baſe of the Muriatic Acid.*

Mr. William Lambe, in an eſſay in the fifth volume of the Mancheſter Memoirs, has attempted to prove that ſulphurated hydrogen is the baſe of the muriatic acid. He obtained ox. muriatic gas by dropping ſulphuric acid upon the reſiduum left after evaporating water which had been impregnated with hepatic gas in which iron and manganeſe had been digeſted. I have performed this experiment, and the reſult is exactly as ſtated by Mr. Lambe.

Two drachms of the filings of bar-iron were placed in twenty-two ounce meaſures of diſtilled water, which had been impregnated with ſulphurated hydrogen gas in Nooth's apparatus. In five days twelve ounce meaſures of inflammable air eſcaped from the water. Six ounces of the clear fluid evaporated to dryneſs, left a reſiduum, conſiſting of dephlogiſticated muriat of iron, which attracted the moiſture of the atmoſphere. A few drops of ſulphuric acid let fall upon it, produced an effervescence, and white clouds of ox. muriatic gas eſcaped, as was very evident from the ſmell, and from the teſts generally uſed to detect the preſence of this gas.

The diſcovery of Mr. Lambe is the beſt which has been made for many years.

METHOD OF RESTORING WRITING WHICH HAS BEEN EFFACED BY OXYGENATED MURIATIC ACID.

As the art of effacing writing by means of this acid might give riſe to acts of fraud, the following letter from the Secretary-general of the French Miniſter of the Interior has been lately publiſhed in the official paper the Moniteur: it is addreſſed to the editor:—" C. Guietand, apothecary of the Grand Hoſpital of Humanity, the ci-devant Hotel-Dieu, has diſcovered the means of reviving writing effaced by oxygenated muriatic acid *. His proceſs has been examined by chemiſts, who have found, that it is eaſy, as well as ſpeedy in its effect, and that it deſerves to be known.

" The miniſter, therefore, has determined that it ſhall be made known by being printed. I herewith tranſmit you an account of the proceſs; and I requeſt that you will inſert it in your next number."

" *May* 26, 1800. (Signed) F. DESPORTES."

The ſulphure of ammonia and the pruſſiat of potaſh are the two ſubſtances which C. Guitand prefers for reviving writing effaced by oxygenated muriatic acid. The diſagreeable ſmell of the ſulphure of ammonia will, no doubt, prevent many perſons from employing it: it has, however, this advantage, that it does not ſtain the paper, and that it makes the writing reappear when ſimply expoſed to its vapour in a cloſe veſſel or under a bell-glaſs. But to produce this effect more ſpeedily, it will be beſt to pour a few drops of it into water, and to dip the paper in it: the effaced writing ſoon re-appears of a dark brown colour, and exceedingly legible.

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* The proceſs for effacing writing by this acid may be ſeen in the Philoſophical Magazine, Vol. II. p. 28.

The pruſſiat of potaſh gives a blue colour to the writing, whether that effaced, or that which has been ſubſtituted for it; which is ſufficient to detect fraud of this kind: it gives to the paper, at the ſame time, a ſlight tint of blue. To employ it, you put into a baſon, or any other deep veſſel, and of a ſuitable ſize, as much water as may be ſufficient for immerſing the leaves of paper which you wiſh to ſubject to examination. You then add about half a thimble-full of the pruſſiat; and, when it has been well mixed with the water, you immerſe in the liquid a leaf of paper. When it has imbibed the liquid well, a few drops of ſulphuric or any other acid, poured into the mixture in ſuch a manner as to render it ſlightly acid, will be ſufficient to make the writing reappear. It is well known that the ſame ſubſtance poſſeſſes the property of reviving old writing if employed in the like manner.

BOTANY.

On ſome mountains in South Pruſſia a plant was diſcovered in autumn laſt which yields a fine beautiful ſilk. It grows on dry mountains, covered with buſhes, to a height of three or four feet; has roots of the ſhape of quitch-graſs; heart-ſhaped leaves, of a light-green colour; a ſoft ſtalk, of the thickneſs of a quill, covered with a firm ſilky baſt; and a fruit ſimilar to that of the ſilk-plant of Syria, with the difference only, that the pods which contain the ſeed and ſilk-wool are more pointed and ſmaller. The ſilk cannot be diſtinguiſhed from that of Syria.

A NEW VOLCANIC ISLAND.

A letter from the celebrated naturaliſt Pallas, dated the Tauride, announces, that on the 5th of September laſt an iſland ſuddenly made its appearance in the ſea of Azof, at the diſtance of 150 toiſes from the ſhore. This phenomenon was preceded by a noiſe like that of thunder, and accompanied with an eruption of ſmoke and flames, the exploſion of which was as loud as that of a large piece of ordnance: at the ſame time a violent ſhock of an earthquake was experienced from Cuban as far as Catrinodan.

VACCINE INOCULATION.

An eſtabliſhment has lately been formed at Paris for the vaccine inoculation. "A trial of the vaccine inoculation," ſays a letter from Paris, dated June 3, "was begun here yeſterday. Thirty children were inoculated with matter received from London, and according to the directions given by Dr. Pearſon. The matter was tranſmitted hither in phials filled with hydrogen gas cloſed by mercury, and covered with a piece of bladder. The greater part of theſe children were inoculated with the matter on lancets, which Dr. Pearſon recommended as moſt efficacious; ſome of them with threads, applied by means of veſicatories; and a ſmall number with matter collected on glaſs. A daily report will be given of theſe trials, which are made at Vaugirard, the houſe of C. Colon, at which the members of the medical committee are to meet every day at one in the afternoon."

The following letter on this ſubject from Mr. Stromeyer, of Hanover, to Mr. Hannehmann, dated 24th March laſt, contains ſome further information reſpecting the ſtate of vaccine inoculation on the Continent:

"This year we have inoculated forty perſons, as well with the vaccine matter received of Dr. Pearſon, as with that from Dr. Jenner; all of whom underwent the diſeaſe properly.

"Betwixt the London and Glouceſter vaccine matter, it appears to me there ſubſiſts an eſſential difference. The London matter produces frequently an eruption of ſmall pimples, but they diſappear within a day or two at furtheſt. Dr. Pearſon calls theſe eruptions *puſtules.* The Glouceſter matter has never produced this effect here; but it frequently *occaſioned ulcerations of the inoculated part, of a tedious and long duration, which the former matter never did*; on account of which I now only make uſe of Dr. Pearſon's vaccine matter.

"The nettle-fever-like eruptions I have obſerved ſeveral times, but *never that ſort of eruption, repeatedly noticed in London, which ſo much reſembles the ſmall-pox.*

"Of all thoſe I have inoculated, none of them were ſeverely ill during the diſeaſe. Their general ſymptoms were, a ſlight fever, depreſſion of ſpirits, and a pale countenance. In a few inſtances, in which the inoculated part attained to a due degree of maturity, and where a ſwelling of the axillary glands was obſerved, no ſigns of any illneſs exiſted, at leaſt no ſymptoms could be obſerved which proved the infection to have acted upon the whole ſyſtem. Theſe I ſhall re-inoculate. Should the reſult be as before, would it be fair to ſuppoſe them ſecured againſt ſmall-pox infection?

" The moſt of our phyſicians here exclaim againſt the vaccine inoculation; and their only weak and almoſt refuted argument is—Are people thus ſecured for all their lifetime againſt the ſmall-pox? Nevertheleſs I have the ſatisfaction to ſee a partiality for it diſplayed by the greater part of the public; and I have already inoculated many noblemen's children, as well as thoſe of other very reſpectable inhabitants of Hanover; and, without doubt, I ſhall always have ſubjects ſufficient for continuing this inoculation.

" On the ninth, tenth, or eleventh day, when the inoculated puſtule is filled with lymph, and the ſurrounding inflammation is complete, I then uſually open it, collect the lymph on cotton thread, and immediately lay ſome more thread upon it, which is removed the following day: all theſe ſerve for future inoculations. After this period I have hitherto deſiſted to repeat collecting more matter from ſuch a puſtule, but wiſh to know how long it might be continued.

" I ſhall eſteem it a great favour if you will lay theſe reſults and queſtions before Dr. Pearſon, and beg he will have the goodneſs to anſwer them.

" In Langenhagen, a village near Hanover, I have inoculated the clergyman's children along with four others; which induced many farmers and peaſants to have their children alſo inoculated.

" From Halle, Halberſtadt, and many other places, I have received applications for vaccine matter; which proves that the continent has many adherents to the vaccine inoculation as well as England.

" Dr. Ballhorn, an intimate and long acquaintance of mine, aſſiſted me in moſt of theſe operations, in order that he might viſit the inoculated patients when my time would not admit of it.

"In Nos. 15 and 16 of the Hanoverian Magazine we have publiſhed our laſt year's inoculations, and we intend continuing the ſame from time to time. With Dr. De Carro, whom Dr. Pearſon knows, and who occupies himſelf greatly with this inoculation, we correſpond.

"Only one of our this year's patients has been inoculated for the ſmall-pox: a puſtule was produced upon the inoculated part, accompanied with a ſlight ſurrounding inflammation, but no other effects whatever."

NEW VOYAGE OF DISCOVERY.

A new voyage of diſcovery to the South Sea has been lately projected by the French. The National Inſtitute have obtained two corvettes for that purpoſe, which are now fitting out at Havre. They are to follow the route purſued by Dentrecaſteaux, and to continue the reſearches made during the late expedition into thoſe ſeas in order to determine ſeveral important points of geography. They will explore the ſouth-eaſt coaſts of New Holland, and bring back ſuch vegetable productions as are likely to anſwer the purpoſes of cultivation in Europe. The command of this new enterpriſe is entruſted to Captain Baudin, who has already diſtinguiſhed himſelf by his nautical talents and knowledge of the natural ſciences, and who has enriched the Muſeum of Natural Hiſtory at Paris with various curious objects which he collected during a former voyage C. Quenot, an able navigator and obſerver; and C. Ciccolini, a ci-devant knight of Malta, who has long applied to aſtronomical purſuits in the College de France, are the aſtronomers propoſed for this expedition.

DISCOVERIES IN AFRICA.

The following particulars have been received reſpecting the expedition of Horneman to the interior parts of Africa: —In the month of September 1798, he ſet out from Cairo, where he was expoſed to great danger from the plague, and proceeded on his journey with the caravan bound to Fezzan. In the oaſis of Seewah he found Brown's obſervations reſpecting the ſmall ancient buildings there, confirmed. He traced out the ground plan of the walls by which they were ſurrounded, in ſuch a manner, that it appears highly probable that theſe ruins are of great antiquity, and perhaps belonged to the temple of Jupiter Ammon. During the courſe of their journey, after leaving this place, the caravan was ſtopped by about a hundred Arabs on horſeback, who inſiſted on Horneman and his fellow-traveller, a native of Cologne, who, after reſiding long in Egypt, had become a Mahometan, being delivered up to them, on a pretence of their being French ſpies; but they ſaved themſelves by repeating Arabic prayers from the Coran. In forty-one days the caravan arrived at Murſok. Here Horneman's fellow-traveller died, and he himſelf was attacked by an endemial fever, which prevented him from continuing his journey with the caravan to Soudan. He therefore reſolved, as ſoon as he was recovered, to remain at Tripoli till the departure of the next caravan for Soudan, and in the mean time to arrange his papers, to make copies of them, and to tranſmit them to Europe by an Engliſh veſſel. In the month of December he intended to return to Murſok, and to wait there for the departure of the next caravan to Fezzan in the ſpring. It appears by his letters, that, as he is now well acquainted with the languages and manners of the Mahometans, he entertains great hopes of being able to accompliſh his expedition in a ſucceſsful manner.

BREWING.

One of the foreign journals announces, that Profeſſor Scherer, of Vienna, has diſcovered that beet-roots which have been ſubjected to the preſs, and deprived of the greater part of their juice, after being dried, and treated in the ſame manner as grain intended to be made into malt, may be employed in the brewing of beer. A fact of a ſimilar kind, which may furniſh ſome uſeful economical hints, is, that very good brandy may be made from gooſeberries: a gentleman of our acquaintance has now in his poſſeſſion ſome brandy procured from the huſks of gooſeberries left in making gooſeberry wine, which is little inferior to the beſt French brandy.

LETTER

LETTER TO THE EDITOR.

"SIR, *Clifton, June* 8, 1800.

"By a number of lately received communications, I find that much greater activity has been lately exerted in trying the effect of nitric acid in the venereal diſeaſe than I ſuppoſed. I had abandoned every idea of publiſhing another ſeparate collection on this ſubject; but ſo many curious facts have been aſcertained by my correſpondents, and the perfect and permanent antiſiphylitic virtue of the acid, when properly managed, ſo entirely eſtabliſhed, that I cannot heſitate to forward the papers, with which I have been favoured, to the preſs. They will be found to contain intereſting obſervations on other ſubjects.

"The printing of Mr. Davy's reſearches on nitrous (gaſeous) oxyd will be finiſhed this week. The volume will contain ample details on the effects produced by this gas when reſpired.

"I am, Sir, yours very reſpectfully,

"*Mr. Tilloch.* THOMAS BEDDOES."

DEATHS.

At Madrid, in January laſt, J. Bapt. Munoz, well known by his Hiſtory of the New World, which has been tranſlated into different languages. This work is not yet completed.—On the 20th of January, at Paris, J. B. Le Roy, Member of the National Inſtitute, Fellow of the Royal Society of London, and of the Society of Philadelphia.—At Harderwyck, in Holland, on the 1ſt of February, in the 32d year of his age, Dr. C. P. Schacht, profeſſor of medicine, botany, chemiſtry, and natural hiſtory.—At Milan, in priſon, during the courſe of the ſame month, C. Barletti, formerly profeſſor of natural philoſophy at Pavia, and author of ſeveral works on natural philoſophy and chemiſtry.—At Pavia, in March, Priſciani, profeſſor of phyſiology, and author of ſeveral works on that ſubject. He has left to the univerſity his collection of preparations reſpecting comparative anatomy, all made by himſelf, and which amount to ſeveral thouſand articles.

THE

PHILOSOPHICAL MAGAZINE.

JULY 1800.

I. *An Account of ſome Experiments on the Fecundation of Vegetables. In a Letter from* THOMAS ANDREW KNIGHT, *Eſq. to the Right Hon. Sir* JOSEPH BANKS, *K. B. P. R. S.*[*]

THE reſult of ſome experiments which I have amuſed myſelf in making on plants, appearing to me to be intereſting to the naturaliſt, by proving the exiſtence of ſuperfœtation in the vegetable world, and being likely to conduce to ſome improvements in agriculture, I have taken the liberty to communicate them to you.

The breeders of animals have very long entertained an opinion, that conſiderable advantages are obtained by breeding from males and females not related to each other. Though this opinion has lately been controverted, the number of its oppoſers has gradually diminiſhed; and I can ſpeak from my own obſervation and experience, that animals degenerate, in ſize at leaſt, on the ſame paſture, and in other reſpects under the ſame management, when this proceſs of croſſing the breed is neglected.

The cloſe analogy between the animal and vegetable world, and the ſexual ſyſtem equally pervading both, induced me to ſuppoſe, that ſimilar means might be productive of ſimilar effects in each; and the event has, I think, fully juſtified this opinion. The principal object I had in view, was to obtain new and improved varieties of the apple, to ſupply the place of thoſe which have become diſeaſed and unproductive, by having been cultivated beyond the period which nature appears to have aſſigned to their exiſtence. But, as I foreſaw that ſeveral years muſt elapſe before the ſucceſs or failure of this proceſs could poſſibly be aſcertained, I wiſhed, in the interval, to ſee what would be its effects on annual plants. Amongſt theſe, none appeared ſo well calculated to anſwer my purpoſe as the common pea; not only becauſe I could obtain many varieties of this plant, of different forms, ſizes, and colours; but alſo, becauſe the ſtructure of its bloſſom, by preventing the ingreſs of inſects and adventitious farina, has rendered its varieties remarkably permanent. I had a kind growing in my garden, which, having been long cultivated in the ſame ſoil, had ceaſed to be productive, and did not appear to recover the whole of its former vigour, when removed to a ſoil of a ſomewhat different quality; on this, my firſt experiment, in 1787, was made. Having opened a dozen of its immature bloſſoms, I deſtroyed the male parts, taking great care not to injure the female ones; and, a few days afterwards, when the bloſſoms appeared mature, I introduced the farina of a very large and luxuriant gray pea into one half of the bloſſoms, leaving the other half as they were. The pods of each grew equally well; but I ſoon perceived that in thoſe into whoſe bloſſoms the farina had not been introduced, the ſeeds remained nearly as they were before the bloſſoms expanded, and in that ſtate they withered. Thoſe in the other pods attained maturity, but were not in any ſenſible degree different from thoſe afforded by other plants of the ſame variety; owing, I imagine, to the external covering of the ſeed (as I have found in other plants) being furniſhed entirely by the female. In the ſucceeding ſpring, the difference, however, became extremely obvious; for the plants from them roſe with exceſſive luxuriance, and the colour of their leaves and ſtems clearly indicated that they had all exchanged their whiteneſs for the colour of the male parent: the ſeeds produced in autumn were dark gray. By introducing the farina of another white variety, (or, in ſome inſtances, by ſimple culture,) I found this colour was eaſily diſcharged, and a numerous variety of new kinds produced, many of which were, in ſize, and in every other reſpect, much ſuperior to the original white kind, and grew with exceſſive luxuriance, ſome of them attaining the height of more than twelve feet. I had frequent occaſion to obſerve in this plant a ſtronger tendency to produce purple bloſſoms; and coloured ſeeds, than white ones: for, when I introduced the farina of a purple bloſſom into a white one, the whole of the ſeeds in the ſucceeding year became coloured; but, when I endeavoured to diſcharge this colour, by reverſing the proceſs, a part only of them afforded plants with white bloſſoms; this part ſometimes occupying one end of the pod, and being at other times irregularly intermixed with thoſe which, when ſown, retained their colour. It may perhaps be ſuppoſed, that ſomething might depend on the quantity of farina employed; but I never could diſcover, in this, or in any other experiment in which ſuperfœtation did not take place, that the largeſt or ſmalleſt quantity of farina afforded any difference in the effect produced.

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[*] From *Tranſactions of the Royal Society of London for* 1799.

The diſſimilarity I obſerved in the offspring afforded by different kinds of farina, in theſe experiments, pointed out to me an eaſy method of aſcertaining whether ſuperfœtation (the exiſtence of which has been admitted amongſt animals) could alſo take place in the vegetable world. For, as the offspring of a white pea is always white, unleſs the farina of a coloured kind be introduced into the bloſſom, and, as the colour of the gray one is always transferred to its offspring, though the female be white, it readily occurred to me, that if the farina of both were mingled, or applied at the ſame moment, the offspring of each could be eaſily diſtinguiſhed.

My firſt experiment was not altogether ſucceſsful; for the offspring of five pods (the whole which eſcaped the birds) received their colour from the coloured male. There was, however, a ſtrong reſemblance to the other male, in the growth and character of more than one of the plants; and the ſeeds of ſeveral, in the autumn, very cloſely reſembled it in every thing but colour. In this experiment I uſed the farina of a white pea, which poſſeſſed the remarkable property of ſhrivelling exceſſively when ripe; and, in the ſecond year, I obtained white ſeeds, from the gray ones above mentioned, perfectly ſimilar to it. I am ſtrongly diſpoſed to believe, that the ſeeds were here of common parentage; but I do not conceive myſelf to be in poſſeſſion of facts ſufficient to enable me to ſpeak with deciſion on this queſtion.

If, however, the female afford the firſt organiſed atom, and the farina act only as a ſtimulus, it appears to me by no means impoſſible, that the exploſion of two veſicles of farina, at the ſame moment, (taken from different plants,) may afford ſeeds (as I have ſuppoſed) of common parentage; and, as I am unable to diſcover any ſource of inaccuracy in this experiment, I muſt believe this to have happened.

Another ſpecies of ſuperfœtation (if I have juſtly applied that term to a proceſs in which one ſeed appears to have been the offspring of two males) has occurred to me ſo often, as to remove all poſſibility of doubt as to its exiſtence. In 1797, that year after I had ſeen the reſult of the laſt-mentioned experiment, having prepared a great many white bloſſoms, I introduced the farina of a white and that of a gray pea, nearly at the ſame moment, into each; and as, in the laſt year, the character of the coloured male had prevailed, I uſed its farina more ſparingly than that of the white one; and now almoſt every pod afforded plants of different colours. The majority, however, were white; but the characters of the two kinds were not ſufficiently diſtinct to allow me to judge with preciſion whether any of the ſeeds produced were of common parentage or not. In the laſt year I was more fortunate: having prepared bloſſoms of the little early frame pea, I introduced its own farina, and immediately afterwards that of a very large and late gray kind, and I ſowed the ſeeds thus obtained in the end of the laſt ſummer. Many of them retained the colour and character of the ſmall early pea, not in the ſlighteſt degree altered, and bloſſomed before they were eighteen inches high; whilſt others, (taken from the ſame pods,) whoſe colour was changed, grew to the height of more than four feet, and were killed by the froſt, before any bloſſoms appeared.

It is evident, that in theſe inſtances ſuperfœtation took place; and it is equally evident, that the ſeeds were not all of common parentage. Should ſubſequent experience evince that a ſingle plant may be the offspring of two males, the analogy between animal and vegetable nature may induce ſome curious conjecture relative to the proceſs of generation in the animal world.

In the courſe of the preceding experiments, I could never obſerve that the character, either of the male or female, in this plant, at all preponderated in the offspring; but, as this point appeared intereſting, I made a few trials to aſcertain it. And as the foregoing obſervations had occurred in experiments made principally to obtain new and improved varieties of the pea, for garden culture, I choſe, for a ſimilar purpoſe, the more hardy varieties uſually ſown in the fields. By introducing the farina of the largeſt and moſt luxuriant kinds into the bloſſoms of the moſt diminutive, and by reverſing this proceſs, I found that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the ſize of the ſeeds produced, and the ſeaſon of maturity, were the ſame, though the one was a very early, and the other a late variety. I had, in this experiment, a ſtriking inſtance of the ſtimulative effects of croſſing the breeds; for the ſmalleſt variety, whoſe height rarely exceeded two feet, was increaſed to ſix feet; whilſt the height of the large and luxuriant kind was very little diminiſhed. By this proceſs, it is evident, that any number of new varieties may be obtained; and it is highly probable, that many of theſe will be found better calculated to correct the defects of different ſoils and ſituations, than any we have at preſent; for I imagine that all we now poſſeſs, have in a great meaſure been the produce of accident; and it will rarely happen, in this or any other caſe, that accident has done all that art will be found able to accompliſh.

The ſucceſs of my endeavours to produce improved varieties of the pea, induced me to try ſome experiments on wheat; but theſe did not ſucceed to my expectations. I readily obtained as many varieties as I wiſhed, by merely ſowing the different kinds together; for the ſtructure of the bloſſom of this plant (unlike that of the pea) freely admits the ingreſs of adventitious farina, and is thence very liable to ſport in varieties. Some of thoſe I obtained were excellent;

lent; others very bad; and none of them permanent. By ſeparating the beſt varieties, a moſt abundant crop was produced; but its quality was not quite equal to the quantity, and all the diſcarded varieties again made their appearance. It appeared to me an extraordinary circumſtance, that in the years 1795 and 1796, when almoſt the whole crop of corn in the iſland was blighted, the varieties thus obtained, and theſe only, eſcaped, in this neighbourhood, though ſown in ſeveral different ſoils and ſituations.

My ſucceſs on the apple (as far as long experience and attention have enabled me to judge from the cultivated appearance of trees which have not yet borne fruit) has been fully equal to my hopes. But, as the improvement of this fruit was the firſt object of my attention, no probable means of improvement, either from ſoil or aſpect, were neglected. The plants, however, which I obtained from my efforts to unite the good qualities of two kinds of apple, ſeem to poſſeſs the greateſt health and luxuriance of growth, as well as the moſt promiſing appearance in other reſpects. In ſome of theſe, the character of the male appears to prevail; in others, that of the female; and in others, both appear blended, or neither is diſtinguiſhable. Theſe variations, which were often obſervable in the ſeeds taken from a ſingle apple, evidently ariſe from the want of permanence in the character of this fruit when raiſed from ſeed.

The reſults of ſimilar experiments on another fruit, the grape, were nearly the ſame as of thoſe on the apple, except that, by mingling the farina of a black and a white grape, juſt as the bloſſoms of the latter were expanding, I ſometimes obtained plants, from the ſame berry, ſo diſſimilar, that I had good reaſon to believe them the produce of ſuperfœtation. By taking off the cups, and deſtroying the immature male parts, (as in the pea,) I perfectly ſucceeded in combining the characters of different varieties of this fruit, as far as the changes of form, and autumnal tints, in the leaves of the offspring, will allow me to judge.

Many experiments, of the ſame kind, were tried on other plants; but it is ſufficient to ſay, that all tended to evince, that improved varieties of every fruit and eſculent plant may be obtained by this proceſs, and that nature intended that a ſexual intercourſe ſhould take place between neighbouring plants of the ſame ſpecies. The probability of this will, I think, be apparent, when we take a view of the variety of methods which nature has taken to diſperſe the farina, even of thoſe plants in which it has placed the male and female parts within the ſame empalement. It is often ſcattered by an elaſtic exertion of the filaments which ſupport it, on the firſt opening of the bloſſom; and its exceſſive lightneſs renders it capable of being carried to a great diſtance by the wind. Its poſition within the bloſſom is generally well adapted to place it on the bodies of inſects; and the villous coat of the numerous family of bees, is not leſs well calculated to carry it. I have frequently obſerved, with great pleaſure, the diſperſion of the farina of ſome of the graſſes when the ſun had juſt riſen in a dewy morning. It ſeemed to be impelled from the plant with conſiderable force; and, being blue, was eaſily viſible, and very ſtrongly reſembled, in appearance, the exploſion of a grain of gunpowder. An examination of the ſtructure of the bloſſoms of many plants, will immediately point out, that nature has ſomething more in view, than that its own proper males ſhould fecundate each bloſſom; for the means it employs are always thoſe beſt calculated to anſwer the intended purpoſe. But the farina is often ſo placed, that it can never reach the ſummit of the pointal, unleſs by adventitious means; and many trials have convinced me, that it has no action on any other part of it. In promoting this ſexual intercourſe between neighbouring plants of the ſame ſpecies, nature appears to me to have an important purpoſe in view; for, independent of its ſtimulative power, this intercourſe certainly tends to confine, within more narrow limits, thoſe variations which accidental richneſs or poverty of ſoil uſually produces. It may be objected, by thoſe who admit the exiſtence of vegetable mules, that, under this extenſive intercourſe, theſe muſt have been more numerous; but my total want of ſucceſs, in many endeavours, to produce a ſingle mule plant, makes me much diſpoſed to believe that hybrid plants have been miſtaken for mules; and to doubt (with all the deference I feel for the opinions of Linnæus and his illuſtrious followers) whether nature ever did, or ever will, permit the production of ſuch a monſter. The exiſtence of numerous mules in the animal world, between kindred ſpecies, is allowed; but nature has here guarded againſt their production, by impelling every animal to ſeek its proper mate; and, amongſt the feathered tribe, when, from perverſion of appetite, ſexual intercourſe takes place between thoſe of diſtinct genera *, it has, in ſome inſtances at leaſt, rendered the death of the female the inevitable conſequence. But, in the vegetable world, there is not any thing to direct the male to its proper female: its farina is carried, by winds and inſects, to plants of every different genus and ſpecies; and it therefore appears to me, (as vegetable mules certainly are not common,) that nature has not permitted them to exiſt at all.

I cannot diſmiſs this ſubject without expreſſing my regret, that thoſe who have made the ſcience of botany their ſtudy, ſhould have conſidered the improvement of thoſe vegetables which, in their cultivated ſtate, afford the largeſt portion of ſubſiſtence to mankind and other animals, as little connected with the object of their purſuit. Hence it has happened, that whilſt much attention has been paid to the improvement of every ſpecies of uſeful animal, the moſt valuable eſculent plants have been almoſt wholly neglected. But, when the extent of the benefit which would ariſe to the agriculture of the country, from the poſſeſſion of varieties of plants which, with the ſame extent of ſoil and labour, would afford even a ſmall increaſe of produce, is conſidered, this this ſubject appears of no inconſiderable importance. The improvement of animals is attended with much expenſe, and the improved kinds neceſſarily extend themſelves ſlowly; but a ſingle buſhel of improved wheat or peas, may in ten years be made to afford ſeed enough to ſupply the whole iſland; and a ſingle apple, or other fruit-tree, may within the ſame time be extended to every garden in it. Theſe conſiderations have been the cauſe of my addreſſing the fore-

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* This is ſaid to be the caſe with the drake and the hen.

going obſervations to you at this time; for it was much my wiſh to have aſcertained, before I wrote to you, whether in any inſtance a ſingle plant can be the offspring of two male parents. The deciſion of that queſtion muſt of neceſſity have occupied two years, and muſt therefore be left to the teſt of future experiment.

II. *Chemical Experiments and Obſervations on the Preparation of Sugar from vegetable Productions found in Europe.*

By SIGISMUND FREDERICK HERMBSTADT[*].

THE chemical examination of vegetable ſubſtances, and the reſearches made reſpecting their admixture and component parts, furniſh ſufficient proofs that the Eaſt and Weſt Indies are not the only countries which nature has bleſſed with ſaccharine plants. Saccharine matter is diffuſed in ſuch abundance throughout the vegetable kingdom, that nothing is neceſſary but perſeverance to diſcover thoſe individual links of the grand chain from which it may be extracted in the greateſt plenty, in the pureſt manner, and at the cheapeſt rate.

Of all the plants hitherto examined for this purpoſe. There is none, without doubt, that approaches ſo near to the ſugar-cane as the maple-tree, particularly the ſugar-maple *acer ſaccharinum*, and the ſilver-maple *acer daſycarpum*. Theſe trees have been employed in North America more than half a century for making ſugar, and during the laſt eight years with very great ſucceſs. I have learned by my own experiments, purpoſely made on various kinds of the maple, and on a pretty large ſcale, for three years, ſince the winter of 1796, not only that they may all be employed with greater or leſs advantage for the production of ſugar, but that the ſugar and ſilver maples which grow in Germany, and even on indifferent ſoil, when their juice is boiled, will produce ſugar fully equal to the raw or moſcavado ſugar of the Weſt Indies, and at ſo cheap a rate, that a pound will not coſt more than four-pence or five-pence. But it may be readily ſeen, that when this labour can be undertaken on a large ſcale, in which caſe one workman, during the proceſs of tapping, might be able to attend 500 trees, and if the liquor could be boiled with coals or turf, a pound of ſuch ſugar would not coſt nearly ſo much. The proceſs of boiling the juice is ſo ſimple that it may be undertaken by every farmer. As a proof of this I ſhall obſerve, that Count Von Podevils, of Guſon, having brought his huntſman to me in order that he might be preſent at the boiling of maple juice, and to learn the method, he ſoon after tried the proceſs himſelf on his maſter's eſtate, and ſent him a pretty large portion of maple ſugar as a ſpecimen. But it appears, from various experiments, that the ſugar and ſilver maples are the propereſt for this purpoſe. As the aſh-leaved maple, *acer negundo*; common maple, *acer campeſtre*; Norway maple, *acer plantoides*; great maple or baſtard ſycamore, *acer pſeudo-platanus*; and all the other ſpecies, are leſs abundant in juice, and as the juice contains leſs ſaccharine matter, the manufacturing of maple ſugar can be expected to become advantageous only when ſufficient plantations of the ſilver and ſugar maples have been formed, and have come to maturity, which will require a period of from twenty-five to thirty years *.

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[*] From *Schriften der Geſellſchaft naturforſchender Freunde zu Berlin*, Vol. II.

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* I cannot omit taking this opportunity to refute ſome ridiculous ideas which have been entertained in regard to the maple. Sometimes it is ſaid that it is apt to be killed by the ſeverity of the froſt in winter; ſometimes that it dies after being tapped; and ſometimes that it is deſtroyed by caterpiliars. In regard to the firſt objection, I ſhall refer to the maple plantations of Count Veltheim at Harbke. The greater part of the ſugar and ſilver maples there are above thirty years old; they withſtood the cold winters of the years 1776, 1788, and 1793; and will ſtill continue to thrive, for the leaſt trace of corruption, not even in the branches, can be obſerved in them. Muſt not theſe trees have been long ago deſtroyed, if they are ſo liable to ſuffer from the effects of froſt? In regard to the ſecond objection, that alſo may be refuted by experience. The maples at Harbke have been tapped two years ſucceſſively without one of them being hurt; on the contrary, they produced bloſſoms and ſeeds the following ſpring. This winter Count Veltheim cauſed them to be tapped, for the third time. In the like manner, ſeveral trunks of the *acer pſeudo-*

platanus

In order, therefore, that Germany, and in particular the Pruſſian ſtates, may not be ſo long deprived of an opportunity of eſtabliſhing manufactories of ſugar from indigenous productions, it would be of great benefit if, among the many ſaccharine plants which nature has beſtowed on Germany, one were diſcovered capable of producing a ſubſtitute for Weſt India ſugar, until plantations of the maple-tree can be formed. This conſideration induced me, while employed in reſearches reſpecting maple ſugar, to begin a ſeries of experiments, the object of which was, to examine what the old and modern chemiſts had done on this ſubject, and to enlarge their diſcoveries by my own. The reſult of my experiments was as follows:

1. *Experiment on Turkiſh Wheat in order to procure Sugar from it.*

Turkiſh wheat, (*zea mays*,) according to Mr. Von Juſti [*], contains a great deal of real ſugar in the knots of the young ſtalks. Mr. Jacquin, of Vienna [†], frequently ſucceeded in preparing ſugar from the ſtalks of Turkiſh wheat; and the ſame thing is aſſerted by Mr. Marabelli in a treatiſe which he publiſhed on this ſubject. The manufacturing of ſugar from the ſtalks of Turkiſh wheat, and eſpecially ſuch as grows in a marſhy ſoil, has been tried alſo in Italy on a large ſcale; but it has been found that the ſugar procured from this production is too dear, when the price of it is compared with

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platanus and *acer plantoides*, in the foreſts of Prince Henry of Pruſſia at Rheinſberg, were tapped laſt winter (1798) without one of them ſuſtaining the leaſt injury. In order to try how far it was poſſible to kill a maple-tree by tapping, I cauſed two trunks to be bored with twenty-four holes each, and when the ſap had ceaſed flowing, the holes not only were left open, but eighteen deep wounds were made in each tree with an axe, and all the branches were lopped off; yet both theſe trees remained ſound, and next ſummer all the wounds had cloſed up of themſelves. The caſe is not the ſame with the birch, which, when tapped, and deprived of a great deal of ſap, immediately dies. The third objection is ſo ridiculous, and ſo contrary to experience, that nothing needs be ſaid on the ſubject.—H.

[*] See his *Oeconom. Schriften*, Part I. p. 397, and Part II. p. 191.

[†] Crell's *Chem. Annalen* for 1794, Vol. I. p. 96.

that of raw ſugar. As my principal object was to be convinced of this by my own obſervation, I made the following experiments:

I ſowed for this purpoſe, in the ſummer of the year 1796, on a pretty good, but ſomewhat marſhy ſoil, a certain quantity of Turkiſh wheat. When the young plants were about ſix inches in height, the leaves, on being chewed, had a ſweetiſh taſte, but the ſtalks, particularly at the joints, had a real ſaccharine taſte. Theſe young plants were cut cloſe to the ground, and, after they had been freed from the leaves and dirt adhering to them, ten pounds of them were bruiſed in a ſtone mortar, and the juice expreſſed. This juice, which weighed three pounds, and which had a ſweet but herbaceous taſte, was clarified with the white of an egg; and the liquor, which was now of a bright yellow wine colour, and which had loſt almoſt entirely its unpleaſant taſte, was inſpiſſated to the conſiſtence of ſyrup, eight ounces of which would have afforded an agreeable ſyrup fit for uſe.

2. *Examination of the Ears of Turkiſh Wheat.*

As the young ears of Turkiſh wheat, when they are juſt beginning to be formed, have a very agreeable ſaccharine taſte, theſe alſo were ſubjected to examination. Ten pounds of them, freed from the ſurrounding leaves, being bruiſed in a ſtone mortar, and then preſſed, gave four pounds of a milky juice, which could not be perfectly clarified by the white of an egg. When evaporated ſlowly to the conſiſtence of ſyrup, I obtained from the above quantity of juice nine ounces of a brown well taſted ſyrup; which, however, was diſtinguiſhed by a greater abundance of ſlimy matter than the former.

3. *Examination of the Stalks of Turkiſh Wheat at a later Period of Growth.*

Twenty pounds of the ſtalks of this plant at a more advanced period of growth being chopped, were bruiſed in a ſtone mortar with a little water, and the juice was expreſſed. This juice, which had an unpleaſant herbaceous and ſomewhat ſharp taſte, when clarified as before, with the white of an egg, and then inſpiſſated, gave twelve ounces of ſyrup, which had a diſagreeable ſaline taſte, and might be conſidered rather as a vegetable extract than a ſyrup.

4. *Experiment made to obtain dry Sugar from Turkiſh Wheat.*

To try how far it was poſſible to obtain cryſtalliſed ſugar from this plant, a quantity of ſyrup prepared from the young ſtalks, and another procured from the ears, were each diſſolved by newly made lime-water, and then boiled ſlowly, during which proceſs a great deal of impurities were thrown up. When the liquors were ſtrained through a woollen cloth, each of them was inſpiſſated ſeparately to the conſiſtence of thick ſyrup, and ſuffered to remain for eight months in a glaſs in a place moderately warm. At the end of that time I found ſmall cryſtals of real ſugar ſhot up in them; but it was with great difficulty they could be ſeparated from the fluid part, the quantity of which was far greater. Each of the ſyrups was therefore inſpiſſated, at a gentle heat, to complete dryneſs, and the dry maſs was then digeſted with ſix parts of rectified ſpirit of wine. The liquor, ſtill warm, being ſpeedily ſtrained through a piece of linen as the undiſſolved ſlimy parts remained behind, real ſugar of a yellowiſh colour cryſtalliſed from the ſpirituous ſolution on cooling. The alcohol was now drawn off from the remaining liquor, and more ſugar was obtained from the reſiduum by ſlow evaporation. I procured in the whole from the ſyrup produced by the young ſtalks, two ounces; and from that produced by the young ears, one and a half ounce of ſugar.

By this it is ſufficiently proved that real ſugar may be obtained both from the young freſh ſtalks and from the young ears of Turkiſh wheat. The ſeparation of it, however, from the gummy and other parts with which it is combined, is attended with ſo much difficulty, and the produce in general is ſo ſmall, that a pound of raw ſugar of this kind will not coſt leſs than a dollar; ſo that, in an economical point of view, no advantage can be expected from the preparation of ſugar in this manner.

5. *Experiment made to obtain Sugar from Siberian Cow-parſnip.*

The Ruſſian or Kamtſchatſdale cow-parſnip, *Heracleum ſphondylium* Linn. *Heracleum Sebiricum* has been long known as a ſaccharine plant. According to Steller, it is the moſt abundant in ſaccharine matter next to the ſugar-cane.* The Ruſſians call it ſweet-herb, and the inhabitants of Kamtſchatka *katſch.* According to Gmelin, it is in nothing different from common bear's breech; but according to others, it forms a peculiar ſpecies *Sphondylium Panaces.* The natives of Kamtſchatka gather the ſtems and large ſtalks of the leaves, and having freed them from the leaves, ſcrape the external rind with muſcle-ſhells, and then leave them to dry in the ſun.

The natives of Kamtſchatka generally chew theſe dried ſtalks in order to extract from them the ſaccharine matter. When the moiſture of the juice has been evaporated in the ſun, the ſurface of the ſtem becomes covered with a white, ſaccharine, mealy ſubſtance, which is ſeparated by ſhaking the ſtalks in leathern bags, and which is then preſerved as powder ſugar. Forty pounds of dried ſtems produce, however, ſcarcely a quarter of a pound of this ſugar, which therefore is ſcarce. The ſtems and roots of this plant are employed alſo by the natives of Kamtſchatka for preparing a kind of brandy †.

As I did not find the ſtems of this plant nearly ſo abundant in ſaccharine matter as thoſe growing in Siberia are ſaid to be, I collected about four pounds of the roots in the end of harveſt: they had a ſweetiſh taſte, ſomething like that of parſnips. I ſcraped off the rind and then dried them, but I could obſerve on them no ſaccharine incruſtation. I therefore raſped the whole four pounds, kneaded the raſpings with water, and expreſſed the juice, which had a ſweetiſh and ſomewhat ſharp taſte. I then boiled it with the white of an egg, by which it was clarified; and having inſpiſſated the liquor, it produced ſix ounces of a brown and not unpleaſant ſyrup, in which, after the end of three months, a kind of brown granulated ſugar had cryſtalliſed. This ſugar, however, was not free from a foreign taſte. That ſugar can be obtained from this plant is therefore proved, but, in an economical point of view, neither the ſugar nor the ſyrup it produces would be advantageous, as both would be too expenſive. It is not improbable that this plant in our climate is leſs abundant in ſugar than that found in Kamtſchatka.

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* Steller's *Reiſen nach Kamtſchatka*, p. 84.

† Gmelin *Flora Sebirica*; Vol. I. p. 214.

6. *Experiment with the Juice of Grapes in order to obtain Sugar.*

The liquor obtained from perfectly ripe grapes, evidently ſhows, by its ſweet taſte, that it contains abundance of ſaccharine matter, but mixed with a great deal of mucilage. In order that I might try how far it was poſſible to procure from it real ſugar, or at leaſt ſyrup, fit for domeſtic purpoſes, I undertook the following experiment:—Eight Berlin quarts of juice obtained from perfectly ripe and ſweet grapes, by merely ſuffering it to diſtil ſpontaneouſly, was mixed with the white of an egg, then boiled and clarified, and afterwards filtered. The clear filtered liquor being evaporated, gave three pounds of a ſyrup not unpleaſant, but which had a ſomewhat ſouriſh taſte. To free it from its acidity it was diſſolved in lime-water, till it appeared by reagents that it contained no more free acid. The liquor was then once more clarified with the white of an egg, and again evaporated, by which means I obtained a very pleaſant ſyrup, but notwithſtanding all my labour I was not able to procure from it cryſtalliſed ſugar. This ſyrup, therefore, while a hogſhead of new wine coſts fifteen dollars, would be too expenſive to be uſed for domeſtic purpoſes.

7. *Experiments with the Juice of the White and Black Birch to obtain Sugar from it.*

Different opinions have been entertained reſpecting the poſſibility of obtaining ſugar from the juice of the birch. As ſome have aſſerted that ſugar may be procured from this liquor, while others have maintained the contrary, I was deſirous to determine the queſtion by my own experience.

According to Stahlhammer's experiments *, eight quarts of the juice of the common white birch *betula alba*, drawn off in the ſpring, gave a ſmall quantity of ſyrup, weaker than that obtained from the maple, but ſuperior to the common brown ſyrup. We are told, on the other hand, by Kalm †, that a great deal of ſugar is prepared from the juice of the black American birch *Betula nigra carpinifolia*, called alſo the ſugar-birch, but that this ſugar is not altogether ſo ſweet as the maple ſugar. By the kindneſs of Count Von Veltheim, at Harbke, I was enabled to obtain a ſufficient quantity of the juice of both theſe kinds of birch, with which I made the following experiments:

The trunks of fifty white birches, of from eight to ten inches in diameter, being pierced in the month of April, which is the moſt favourable period, gave in the courſe of four days 140 Berlin quarts of ſap, which by inſpiſſation produced two and a half pounds of brown ſyrup, which had an unpleaſant taſte, and from which no cryſtalliſable ſugar could be obtained.

On the other hand, ten trunks of the black birch which had been tapped, gave in the courſe of ten days fifty quarts of ſap, which produced by inſpiſſation one pound and a half of very good ſyrup; which, however, was far inferior to that obtained from the maple, though it was better than common ſyrup, and therefore I am induced to believe that Stahlhammer for his experiments muſt have employed the black birch. In the courſe of four months a conſiderable portion of ſugar had cryſtalliſed from the juice of this birch. It is therefore proved that real ſugar may be obtained from the ſap of the black birch, but inferior in quality to that of the maple-tree, and much more expenſive.

8. *Experiments with the White Beet* Beta cicla alba.

The late Margraf made various experiments, above fifty years ago, with ſeveral kinds of beet-root, in order to try how far it was poſſible to procure ſugar from them, and according to his own account he ſucceeded in obtaining from the *Beta cicla* not only good ſyrup, but ſugar, fit for domeſtic purpoſes. Two pounds of freſh roots gave on drying half a pound; and from this quantity he produced, after extracting the juice by means of rectified ſpirit of wine, half an ounce of good ſugar.

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* *Tranſactions of the Swediſh Academy of Sciences*, Vol. XXXV. p. 335.

† *Ibid.* Vol. XIII. p. 151.

In conſequence of theſe experiments I took 130 roots, which weighed ſixty-eight pounds, and having bruiſed them and expreſſed the juice, I poured tepid water over the reſiduum, expreſſed the juice again, and obtained altogether twenty quarts of juice. This juice I evaporated to one-third, then added to it twenty quarts of freſh lime-water, and boiled the whole for half an hour. The clear liquor, which was of a yellow wine colour, being filtered when cool, and then inſpiſſated, gave ſix pounds of a pleaſant browniſh-yellow tranſparent ſyrup. A part of this ſyrup I poured into a ſomewhat deep glaſs evaporating veſſel, in the inſide of which I placed ſome glaſs rods, and ſuffered the liquor to evaporate in a moderately warm place for ſixteen weeks, at the end of which I found the glaſs rods covered with cryſtals of ſugar, ſome of which were as large as lentils, and others as peas, and which had a perfect reſemblance to yellow ſugar-candy. As the quantity of theſe beets which I employed coſt half-a-crown, and as the reſt of the expenſe amounted only to about eightpence, a pound of ſuch ſyrup would coſt no more than ſixpence or ſeven-pence, which ſufficiently proves that it might be uſed as a ſubſtitute for common ſugar ſyrup. Sugar alſo fit for domeſtic purpoſes might be made from it; and though it would be inferior to maple ſugar, and though the proceſs for obtaining it would be ſomewhat tedious, it might be uſed with advantage when we conſider the high price of common ſugar.

[To be concluded in the next Number.]

III. *Obſervations on the Evening and Morning Dew*; *in a Letter addreſſed to* C. HASSENFRATZ, *of the Polytechnic School. By* C. A. PRIEUR*.

AS you requeſt me to give you in writing the ſubſtance of what I ſaid a few days ago reſpecting the phenomena of the evening and morning dew, I ſhall endeavour to gratify your wiſhes; being as deſirous as you are, that ſome utility may reſult from it to the young citizens whom you inſtruct.

The evening and morning dew occur to us ſo frequently, and in ſo many climates, that there is reaſon to be aſtoniſhed that philoſophers ſhould have hitherto paid ſo little attention to theſe phenomena, and been contented with the vague explanations given on the ſubject. In the year 1788, I often had occaſion to ride out in the morning and evening; and being thus expoſed to the impreſſions of the dew, I reflected particularly on the circumſtances of theſe phenomena, which have been as yet ſo little examined. I well knew that moiſture depoſited on bodies placed in the open air at the time of ſunſet, was not the ſame as that ſeen afterwards on theſe bodies at the period of ſunriſe: that conſequently there was an interruption in the phenomenon; an evaporation of the dew or water which appeared in the evening, and a new production of water in the morning. I was acquainted alſo with this partial explanation in regard to the evening dew, *viz.* that the moiſture ſeen in the evening ariſes from the air being no longer able to retain the water which it held in ſolution during the day. But whence that wind which always proceeds from the quarter where the ſun is, and which always accompanies this precipitation of water? And beſides, how happens it, that when the ſun is about to appear above the horizon, and even after it has been ſomewhat heated by his preſence, there ſhould be occaſioned a greater cold, a ſtronger wind, and a more abundant precipitation of water than in the evening? To ſolve this difficulty I had recourſe to thoſe fundamental principles by the help of which Monge has explained, in ſo ingenious a manner, the greater part of the phenomena of meteorology; and I ſoon conceived that they were ſufficient for the object in queſtion.

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* From *Journal de l'Ecole Polytechnique*, Vol. II.

The principles from which we muſt ſet out, and which it is here proper to call to mind, are the three following:

1ſt, The air, every thing elſe being equal, diſſolves more water in proportion as it is denſer, that is to ſay, more compreſſed.

2d, It diſſolves a greater quantity alſo in proportion as its temperature is more elevated.

3d, The denſity or ſpecific gravity of air which holds water in ſolution, is leſs than the ſpecific gravity of pure air, the preſſure and temperature being equal; and this ſpecific gravity is leſs according as the quantity of water diſſolved in the air is greater.

It muſt, in the next place, be admitted, that the changes of preſſure and weight which may take place in ſome columns of the atmoſphere, muſt neceſſarily diſturb its equilibrium, and produce in it movements or currents.

Theſe baſes being laid down, let us ſuppoſe for a moment, to render our examination eaſier, that the earth, deprived of its rotary motion, is in an immoveable ſtate before the ſun; and let us ſet aſide alſo the conſideration of all local influence, or in general all cauſes that tend to perturb the regularity of effects. In this ſtate of things what will take place in our atmoſphere? The air expoſed to the rays of the ſun will be heated, and particularly that part of it near the earth, on account of its greater denſity and the reverberation of the planet. The air thus heated will acquire a greater diſſolving power, and will indeed diſſolve a great deal of water if there be any in contact with it. It will take up water therefore from the ſea, from lakes, rivers, and ponds, and from reſervoirs, the ſurface of which is expoſed to its action: it will even abſorb part of the moiſture with which the earth may be impregnated.

Let us now conſider what will take place in a vertical column aſſumed in the atmoſphere, and firſt in one lying directly under the ſun. The air being heated at its lower part, and becoming ſpecifically lighter by diſſolving water, will riſe and be replaced by the adjacent air. A current, therefore, will be eſtabliſhed in the column from the bottom upwards. Were this column inſulated from the neighbouring columns, as it would be if contained in a vertical tube, in proportion as the inferior air charged with water roſe, the upper air would deſcend; would become charged with water in its turn; and would aſcend, and be continually replaced without interruption. Beſides, the air holding water in ſolution, when it attained to a certain height, and experiencing there a colder temperature with a leſs degree of preſſure, would become ſuper-ſaturated, and would abandon its exceſs of water. There would thus be formed a fog or cloud, which might continue to aſcend in conſequence of the movement it had acquired, but which, when accumulated to a certain point, would fall down in rain. It may readily be conceived that this tranſlation of air from a lower to a higher, and from a higher to a lower ſituation, would be performed by reciprocal infiltration, or by a current in both directions regularly maintained.

But things are not altogether ſo in ſuch a vertical column, becauſe in reality it is not ſeparated from the reſt. The latter is ſubject to the ſame effects, only that they are leſs in proportion as theſe columns are removed from the direction of the ſun: the heat, ſolution of water, and aſcenſional force, go on decreaſing the further they are diſtant from that direction. If the ſurface of the earth, therefore, be ſuppoſed a plane, it will be neceſſary to repreſent the air which riſes as a ſort of cone, with its ſummit directed towards the heating luminary; and on the other hand, as the abſolute gravity of each column is increaſed by the whole of the water it diſſolves, the equilibrium requires that there ſhould be a diſcharge from each, into the lateral ones; a circumſtance which muſt evidently be effected where there is the leaſt preſſure. Thus, on the ſame ſuppoſition of the earth being a plane, we ſhould ſee the ſuperior air deſcend and force itſelf along the ſides of the cone above mentioned, producing, by this oblique motion at the ſurface of the earth, a current proceeding in every direction from the quarter of the ſun;

and this current would be increaſed by the vacuum formed at thoſe places where the air, ſufficiently elevated and cooled, precipitated its ſuperabundant water.

We need only to modify this image a very little to make it correſpond with the contour of our globe. The conical ſurface will be converted into a ſpherical ſegment incloſing the enlightened portion of the earth, and according to the curvature of which, the before-mentioned currents will be eſtabliſhed. This curvature will be ſeen in the annexed figure, (Plate III.) which repreſents the globe of the earth placed in the centre of a circular ſtratum ſuppoſed to be occupied by the atmoſphere. The circumference of the earth contains twenty-four diviſions, from which ariſe as many perpendiculars to its ſurface, in order to give an idea of the variations of the phenomena from hour to hour. The ſun, placed towards S, in the prolongation of the line TS, is ſuppoſed to have his whole maſs united in his centre, which can do no hurt to the object which we have here in view. In the laſt place, the curve ABC, traced out at random becauſe its law is unknown, will be ſufficient to give an idea of the effects which we are deſirous of illuſtrating.

Such, then, will be the reſult of the circumſtances ſuppoſed: Water diſſolved by the inferior air in the part expoſed to the ſun; an aſcending movement of the air in that part; deſcending currents diverging from all ſides, and prolonging themſelves over the earth; theſe currents carrying with them water which they have taken from the heated columns that riſe, and which are forced off in a lateral direction; this water thus carried away precipitated, either becauſe the air, coming from the upper regions, is too cold to keep it diſſolved; or on account of the diminution of preſſure ariſing from the weight of the columns being leſs in proportion to their diſtance from that immediately under the ſun; or in conſequence of the mixture of currents with the atmoſpheric air, which they traverſe at the ſurface of the earth when that air is colder; and, in the laſt place, the earth, and all bodies which the currents touch, moiſtened by the water precipitated.

Every one muſt here perceive the evening and morning dew, with the wind and the cold which accompany them; but a few remarks are ſtill neceſſary to complete the deſcription. We ſhall firſt obſerve, that the deſcending currents are prolonged until the reſiſtance of the air which they traverſe has entirely deſtroyed their motion. Beſides, we ſhall ſee directly under the ſun a circular ſpace very much heated, and which does not exhibit to the inhabitants of that part of the earth the phenomenon we are attempting to deſcribe. Leaving this part, in proportion as the ſun appears in a more oblique poſition, we ſhall arrive at a region of leſs heat, where we ſhall begin to perceive a wind coming from the quarter of the ſun, accompanied with a precipitation of water. This region forms a ring around the circular ſpace before mentioned. In a word, by removing from the internal edge of this ring, the wind is found ſtronger, the cold more ſenſible, and the precipitation of water more abundant; effects which ſtill decreaſe at a certain diſtance, and ceaſe entirely on that part of the earth diametrically oppoſite to the ſun.

Thus, according to the hypotheſis of the immobility of our globe in regard to the ſun, there would be on that planet a very extenſive region continually ſubject to the phenomenon of morning and evening dew. Either of theſe expreſſions will denote the phenomenon according as the obſerver ſuppoſes himſelf placed to the eaſt or weſt of the ſun, ſince the one takes place at the riſing of that luminary, and the other at his ſetting.

Let us now return to the real ſtate of things, and reſtore to the earth its diurnal rotation. In that caſe it will ſucceſſively preſent different parts of its ſurface to the phenomena above deſcribed. Thoſe who inhabit that part where the ſun is deſcending towards the horizon, will ſoon ſee the evening dew appear, accompanied with a freſhening eaſt wind; they will ſee theſe phenomena increaſe more and more until after ſunſet, when the effect will be diminiſhed, and at laſt ceaſe altogether. During the night the moiſture depoſited by the evening dew will be evaporated, if the atmoſphere be not already too much charged with water, and no traces of it will remain. Next morning at break of day, the phenomenon will be renewed on the eaſt ſide of the obſerver, with the ſame circumſtances as the preceding evening, and with the ſame gradations and the ſame effect when the ſun is ſtill below the horizon; but with this very remarkable difference, that the effects will be much ſtronger than thoſe of the evening: there will therefore be more wind, more moiſture depoſited, and a more ſenſible cold. The reaſon of this is, that in the evening the precipitation of water, the wind, and the cold, which accompany it, muſt be diminiſhed, becauſe the whole takes place in the neighbourhood and by the mixture of air, which the ſun has heated during the day; while in the morning the coolneſs of the night-air leaves or gives to the phenomenon a little more effect.

We ſee alſo that in the two temperate zones, where there is a greater difference between the temperature of ſummer and winter, and where the length of the days and nights vary a great deal, the effects of the evening and morning dew will be varied and irregular. In ſummer, if the air has diſſolved more water during the day, the diſtillation of dew takes place in air ſtrongly heated, and that of the morning dew in air which the ſhort duration of the night has cooled only to a certain point: in that caſe, the ſolution of water is conſiderable, and the precipitation of dew leſſened. In winter, on the other hand, the cauſe of the ſolution is leſs, but that of precipitation greater. Local circumſtances, and particularly the neighbourhood of water, muſt alſo have an influence on theſe effects. Fine weather increaſes them, and renders them more ſenſible, but cloudy weather leſſens or annuls them.

Under the torrid zone a greater equality prevails between the days and the nights, ſince at the equator they conſiſt of twelve hours each throughout the year. The difference of temperature in ſummer and winter is leſs; that of the day in compariſon of the night is greater than in our latitude, the heat of the day is far more intenſe, and the ſky is almoſt always ſerene and unclouded. It ought thence to follow, that the evening and morning dew will be more ſenſible in ſuch a ſcorched climate than in any other part of the earth. This has indeed been obſerved by travellers. In Egypt, Turkey,

Aſia,

Aſia, the Antilles, and Mexico, and on board ſhips which navigate between the tropics, evening and morning dew have been obſerved in ſuch abundance, that bodies expoſed to them ſeemed as wet as if they had been drenched with rain.

But a conſequence of very great importance, which I think may be drawn from theſe phenomena, is, that they muſt have a great influence on the production and duration of the trade-winds; for the air of the torrid zone being excited to motion almoſt regularly every day, in two oppoſite directions, by forces very different in quantity, muſt be impelled to aſſume and to preſerve a movement in the direction of the ſtrongeſt, which is here the eaſt wind, or that which brings with it the dew of the morning. This cauſe muſt be more effectual, as it acts in the inferior part of the atmoſphere, and as, by affecting the denſeſt part of the air, it is more capable of eſtabliſhing a current in the whole maſs.

Under the frigid zone, where the ſun, particularly during winter, ſcarcely riſes above the horizon in the courſe of the whole day, the precipitation of water will be conſiderable on account of the coldneſs of the climate; the country will be involved in a thick fog, which will be ſcarcely diſperſed on the approach of ſummer; and when it takes place in ſummer, it will extend a great way into the temperate zone. Hence, therefore, we obſerve in our own country, during that ſeaſon, conſiderable fogs, which are often ſo thick as to ſuffer only a faint light to penetrate through them even at noon.

It would be curious to exhibit here a ſeries of correct obſervations on the circumſtances which accompany the evening and morning dew, at different times of the year, and in different countries; on the hour at which theſe phenomena begin, and on the ſuſpenſion or irregularity by which they are affected in different local ſituations; but if the generality of the cauſe to which they are here aſcribed can excite the attention of philoſophers, the intereſting taſk which I have pointed out will no doubt be ſoon accompliſhed.

IV. *Proceſs employed by Profeſſor* LAMPADIUS *for extracting Sugar from White Beet-root*[*].

In the month of January 1799, Profeſſor Lampadius, having learned that Mr. Achard had extracted ſugar from the white beet, *Beta cicla* Linn., reſolved to try an experiment of the like kind, and from a quintal of beet-roots obtained four pounds of ſugar by the following proceſs:—He took a hundred pounds of the roots, peeled them, and having freed them from the ligneous parts, raſped them and expreſſed the juice: the quantity of the juice obtained was 44 pounds. He put the juice into a copper pan, brought ſome charcoal to a ſtate of ignition in the fire, broke it into ſmall fragments of the ſize of a pea, throwing aſide the duſt, which would have coloured the ſugar, and put forty-four ounces of it into the juice. He then boiled the whole for a quarter of an hour, ſtrained it through a piece of flannel cloth, and, after being filtered, placed it again over the fire to bring it to the conſiſtence of ſyrup. It is on this operation that the ſucceſs of the cryſtalliſation chiefly depends. When the juice had acquired the proper conſiſtence, he put it into a cool place and left it there for the ſpace of fifteen days: a cryſtalliſation was produced, and at the end of that time the juice had become like thick ſyrup, and of a brown colour. On taking up a little of it between the fingers, the cryſtals were felt in the form of ſmall grains. Profeſſor Lampadius then ſtrained the ſyrup through a piece of linen cloth, ſqueezing it very cloſely. The cryſtals which remained behind were put into lime-water mixed with a pound of ox's blood, the whole was placed over the fire, and during the ebullition care was taken to ſkim the mixture well. It was afterwards filtered through a piece of flannel, and left to cryſtalliſe. At the end of forty-eight hours, being thrown upon a linen cloth, the cryſtals which remained were not of ſo brown a colour as thoſe firſt obtained, and appeared to be of a larger ſize. They were again boiled in lime-water with a pound of milk; the matter which floated at the ſurface was ſkimmed off, and the cryſtals obtained were blue. They were thrown into a mould after being moiſtened, and produced a loaf of pretty white ſugar, weighing four pounds, and in taſte perfectly ſimilar to that of common ſugar.

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[*] From the *Journal de Phyſique*, Prairial, an. 8.

In February, Profeſſor Lampadius made a ſecond trial: the juice in this caſe was not altogether ſo white as the former, and he obtained from it only three pounds and a half of ſugar. From the different remains and ſyrups which he procured by filtration, he obtained each time ſeven pounds of a kind of rum or very ſtrong ſpirit. Encouraged by this ſucceſs, Profeſſor Lampadius and a rich citizen of Freyberg named Kanitzki, have eſtabliſhed a large manufactory of this kind of ſugar. The firſt reſults of their undertaking have been already publiſhed.

V. *On a new fulminating Mercury.* *By* EDWARD HOWARD, *Eſq. F.R.S.*

[Concluded from Page 35.]

SECTION XVI.

HITHERTO, as much only has been ſaid of the gas, which is ſeparated from the mercurial powder by dilute ſulphuric acid, as was neceſſary to identify it with that into which the ſame acid can reſolve the nitrous etheriſed gas; I have further to ſpeak of its peculiarity *.

The characteriſtic properties of the inflammable gas, ſeem to me to be the following:

1ſt, It does not diminiſh in volume, either with oxygen or nitrous gas.

2dly, It will not explode with oxygen by the electric ſhock in a cloſe veſſel.

3dly, It burns like hydro-carbonat, but with a bluiſh green flame. And,

4thly, It is permanent over water. (*Section* XII.)

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* It muſt be firſt noticed, that it is never pure when obtained from the nitrous etheriſed gas; nor am I aware how it is to be purified, unleſs the nitrous gas could be taken from it without being converted into nitrous acid; for, by that acid, it would probably be itſelf converted into nitrous gas.

It is of courſe either not formed, or is convertible into nitrous gas, by the concentrate nitric and muriatic acids; becauſe, by thoſe acids, no inflammable gas was extricated from the powder.

Should this inflammable gas prove not to be a hydro-carbonat, I ſhall be diſpoſed to conclude that it has nitrogen for its baſis; indeed, I am at this moment inclined to that opinion, becauſe I find that Dr. Prieſtley, during his experiments on his dephlogiſticated nitrous air, once produced a gas which ſeems to have reſembled this inflammable gas, both in the mode of burning, and in the colour of the flame.

After the termination of the common ſolution of iron in ſpirit of nitre, he uſed heat, and got, ſays he*, "ſuch a kind of air as I had brought nitrous air to be, by expoſing it to iron, or liver of ſulphur; for, on the firſt trial, a candle burned in it with a much enlarged flame. At another time, the application of a candle to air produced in this manner, was attended with a real though not a loud exploſion; and, immediately after this, a *greeniſh coloured flame deſcended from the top to the bottom of the veſſel* in which the air was contained. In the next produce of air, from the ſame proceſs, *the flame deſcended blue* and very rapid, from the top to the bottom of the veſſel."

Theſe greeniſh and blue coloured flames, deſcending from the top to the bottom of the veſſel, are preciſely deſcriptive of the inflammable gas ſeparated from the powder. If it can be produced with certainty by the repetition of Dr. Prieſtley's experiments, or ſhould it by any means be got pure from the nitrous etheriſed gas, my curioſity will excite me to make it the object of future reſearch; otherwiſe, I muſt confeſs, I ſhall feel more diſpoſed to proſecute other chemical ſubjects: for, having reaſon to think that the denſity of the acid made a variation in the product of this gas, and having never found that *any* acid, however denſe, produced an immediate exploſion, I once poured ſix drams of concentrate acid upon fifty grains of the powder. An exploſion, nearly at the inſtant of contact, was effected: I was wounded ſeverely, and moſt of my apparatus deſtroyed. A quantity, moreover, of the gas I had previouſly prepared, was loſt by the inadvertency of a perſon who went into my laboratory, whilſt I was confined by the conſequences of this diſcouraging accident. But, ſhould any one be deſirous of giving the gas a further examination, I again repeat, that as far as I am enabled to judge, it may with ſafety be prepared, by pouring three drams of ſulphuric acid, diluted with the ſame quantity of water, upon 50 grains of the powder, and then applying the flame of a candle until gas begins to be extricated. The only attempt I have made to decompoſe it, was by expoſing it to copper and ammoniac; which, during ſeveral weeks, did not effect the leaſt alteration.

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* Prieſtley on Air, Vol. II. p. 58. Birmingham, 1790.

SECTION XVII.

I will now conclude, by obſerving, that the fulminating mercury ſeems to be characteriſed by the following properties:

It takes fire at the temperature of 368 Fahrenheit; explodes by friction *, by flint and ſteel, and by being thrown into concentrate ſulphuric acid. It is equally inflammable under the exhauſted receiver of an air-pump, as ſurrounded by atmoſpheric air; and it detonates loudly, both by the blow of a hammer, and by a ſtrong electrical ſhock.

Notwithſtanding the compoſition of fulminating ſilver, and of fulminating gold, differ eſſentially from that of fulminating mercury, all three have ſome ſimilar qualities. In tremendous effects, ſilver undoubtedly ſtands firſt, and gold perhaps the laſt. The effects of the mercurial powder and of gunpowder admit of little compariſon. The one exerts, within certain limits, an almoſt inconceivable force: its agents ſeem to be gas and caloric, very ſuddenly ſet at liberty, and both mercury and water thrown into vapour. The other diſplays a more extended but inferior power: gas and caloric are, comparatively ſpeaking, liberated by degrees; and water, according to Count Rumford, is thrown into vapour †.

Hence

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[*] Conſequently it ſhould not be incloſed in a bottle with a glaſs ſtopper.

[†] See Philoſophical Tranſactions for the year 1797, p. 222.

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Hence it ſeems that the fulminating mercury, from the limitation of its ſphere of action, can ſeldom if ever be applied to mining; and, from the immenſity of its initial force, cannot be uſed in fire-arms, unleſs in caſes where it becomes an object to deſtroy them; perhaps, where it is the practice to ſpike cannon, it may be of ſervice, becauſe, I apprehend, it may be uſed in ſuch a manner as to burſt cannon without diſperſing any ſplinters.

The inflammation of fulminating mercury by concuſſion, offers nothing more novel or remarkable, than the inflammation, by concuſſion, of many other ſubſtances. The theory of ſuch inflammations has been long ſince expoſed by the celebrated Mr. Berthollet, and confirmed by Meſſieurs Fourcroy and Vauquelin: yet, I muſt confeſs, I am at a loſs to underſtand, why a ſmall quantity of mercurial powder, made to detonate by the hammer, or the electric ſhock, ſhould produce a report ſo much louder than when it is inflamed by a match, or by flint and ſteel. It might at firſt be imagined, that the loudneſs of the report could be accounted for, by ſuppoſing the inſtant of the inflammation, and that of the powder's confinement between the hammer and anvil, to be preciſely the ſame; but, when the electrical ſhock is ſent through or over a few grains of the powder, merely laid on ivory, and a loud report is the conſequence, I can form no idea of what cauſes ſuch a report.

The operation by which the powder is prepared, is perhaps one of the moſt beautiful and ſurpriſing in chemiſtry; and it is not a little intereſting to conſider the affinities which are brought into play. The ſuperabundant nitrous acid of the mercurial ſolution, muſt firſt act on the alcohol, and generate ether, nitrous etheriſed gas, and oxalic acid. The mercury unites to the two laſt in their naſcent ſtate, and relinquiſhes freſh nitrous acid, to act upon any unaltered alcohol. The oxalic acid, although a prediſpoſing affinity ſeems exerted

The hard black ſubſtance mentioned by the Count as remaining after the combuſtion of gunpowder, muſt, I believe, have been an alkaline ſulphuret, mixed chiefly with ſulphit and carbonat of potaſh. The conjecture that it is white when firſt formed, is certainly juſt, as my experiment with the glaſs globe evinced.

in favour of its quantity, is evidently not formed faſt enough to retain all the mercury; otherwiſe, no white fumes, during a conſiderable period of the operation, but fulminating mercury alone, would be produced.

Should any doubt ſtill be entertained of the exiſtence of the affinities which have been called prediſpoſing or conſpiring, a proof that ſuch affinities really exiſt, will, I think, be afforded, by comparing the quantity of oxalic acid, which can be generated from given meaſures of nitrous acid and alcohol, with the intervention of mercury, and the intervention of other metals. For inſtance, when two meaſured ounces of alcohol are treated with a ſolution of 100 grains of nickel in a meaſured ounce and a half of nitrous acid, little or no precipitate is produced; yet, by the addition of oxalic acid to the reſiduary liquor, a quantity of oxalat of nickel, after ſome repoſe, is depoſited. Copper affords another illuſtration: 100 grains of copper, diſſolved in a meaſured ounce and a half of nitrous acid, and treated with alcohol, yielded me about 18 grains only of oxalat; although cupreous oxalat was plentifully generated, by dropping oxalic acid into the reſiduary liquor. About 21 grains of pure oxalic acid ſeem to be produced, from the ſame materials, when 100 grains of mercury are interpoſed. (See Section XIV.) Beſides, according to the Dutch paper, more than once referred to, acetous acid is the principal reſidue after the preparation of nitrous ether. How can we explain the formation of a greater quantity of oxalic acid, from the ſame materials, with the intervention of 100 grains of mercury, than with the intervention of 100 grains of copper, otherwiſe than by the notion of conſpiring affinities, ſo analogous to what we ſee in other phænomena of nature?

I have attempted, without ſucceſs, to communicate fulminating properties, by means of alcohol, to gold, platina, antimony, tin, copper, iron, lead, zinc, nickel, biſmuth, cobalt, arſenic, and manganeſe; but I have not yet ſufficiently varied my experiments, to enable me ſpeak with abſolute certainty. Silver, when 20 grains of it were treated with nearly the ſame proportions of nitrous acid and alcohol as 100 grains of mercury, yielded, at the end of the operation, about three grains of a gray precipitate, which fulminated with extreme violence. Mr. Cruickſhank had the goodneſs to repeat the experiment: he diſſolved 40 grains of ſilver in two ounces of the ſtrongeſt nitrous acid diluted with an equal quantity of water, and obtained (by means of two ounces of alcohol) 60 grains of a very white powder, which fulminated like the gray precipitate above deſcribed. It probably combines with the ſame principles as the mercury, and of courſe differs from Mr. Berthollet's fulminating ſilver, alluded to in page 230. I obſerve that a white precipitate is always produced in the firſt inſtance, and that it may be preſerved, by adding water, as ſoon as it is formed; otherwiſe, when the mother liquor is abundant, it often becomes gray, and is re-diſſolved.

P. S. Since the preceding pages were written, I have been permitted, by the Right Honourable Lord Howe, Lieutenant General of the Ordnance, to make the following trials of the mercurial powder, at Woolwich, in conjunction with Colonel Blomefield and Mr. Cruickſhank *.

Experiment 1. From the manner in which the ſcrew of the gun-breech, mentioned in Section V. had acted on the barrel, it was imagined, that by burſting an iron caſe, exactly fitted to the bore of a cannon, its ſudden enlargement might make many flaws, and ſplit the piece, without diſperſing any ſplinters. In conformity to this opinion, a caſt iron caſe was conſtructed, with a cylindrical chamber, of equal length and diameter, calculated to hold 3¼th ounces troy of the mercurial powder. The caſe, being firmly ſcrewed together, was charged through its vent-hole, and introduced into a twelve-pounder carronade, the bore of which it exactly fitted. The powder was then inflamed, with proper precautions. The gun remained entire, but the caſe divided: the portion forming the upper ſurface of the chamber was expelled in one maſs; that adjoining the breech, which conſtituted the reſt of the chamber, was cracked in every direction, and in part crumbled; yet it was ſo wedged into ſome indentations which the exploſion had made in the ſides of the piece, that the fragments were not removed without great labour.

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* It is with pleaſure I take this opportunity of acknowledging the civil attention I received from the different officers.

Exp. 2. Another caſt iron caſe was prepared, of the ſame ſize as the former, with a chamber alſo cylindrical, but wrought in a tranſverſe direction, and of a greater length than diameter; the thickneſs of metal at each extremity not being more than a quarter of an inch. This caſe was filled with nearly five ounces troy of the mercurial powder, and placed in the ſame carronade. Three twelve-pound ſhot were next introduced, and brought into cloſe contact with the upper ſurface of the caſe, as well as with each other. The gun a ſecond time withſtood the exploſion: the caſe was divided, acroſs the middle of the chamber, into two equal parts; that adjoining the breech was, as in the former experiment, much flawed, and left immoveable; that neareſt to the muzzle was alſo much flawed, but driven out with the ſhot. All the three ſhot were broken; the two lower being divided into ſeveral pieces, and the upper one cracked through the centre.

The report was ſo feeble, in both experiments, that an inattentive perſon, I am confident, would not have heard it at the diſtance of two hundred yards.

Exp. 3. It was found ſo difficult to extract the fragments of the caſe remaining in the carronade after the laſt experiment, that a channel was drilled through them to the vent-hole of the piece. It was then charged with ſix ounces troy of the mercurial powder, made up as a cartridge, which did not occupy above one-half of the diameter of the bore. A wad was placed over the powder, dry ſand ſuperadded, to fill all vacuities, and the gun filled to the muzzle with two twelve-pound ſhot. A block of wood was ſet at a ſmall diſtance, to receive the impreſſion of the ſhot, and the powder was inflamed as uſual. The carronade ſtill reſiſted. One of the ſhot was ſplit into two pieces; and the block of wood was driven to a conſiderable diſtance, but not penetrated by the ſhot above the depth of one inch. The report was ſomewhat louder than the former ones. In all three inſtances, a conſiderable recoil evidently took place. I preſume, therefore, that in the firſt experiment related in the fifth ſection, there muſt have been a recoil, though too trifling to be obſerved; and, in the inſtances where the gun and the proof were burſt, it was not ſo much to be expected.

Exp. 4. Finding that the carronade, from the great comparative ſize of its bore to that of its length, required a larger quantity of mercurial powder to burſt it than we were provided with, we charged a half-pounder ſwivel with an ounce and a half avoirdupois of the mercurial powder, (the ſervice charge of gunpowder being three ounces,) and a half-pound ſhot between two wads. The piece was deſtroyed from the trunnions to the breech, and its fragments thrown thirty or forty yards. The ball penetrated five inches into a block of wood ſtanding at about a yard from the muzzle of the gun; the part of the ſwivel not broken, was ſcarce, if at all, moved from its original poſition.

Exp. 5. One ounce avoirdupois of the mercurial powder, incloſed in paper, was placed in the centre of a ſhell 4,4 inches in diameter, and the vacant ſpace filled with dry ſand.

The ſhell burſt by the exploſion of the powder, and the fragments were thrown to a conſiderable diſtance. The charge of gunpowder employed to burſt ſhells of this diameter, is five ounces avoirdupois.

Exp. 6. A ſea grenade, 3,5 inches diameter, charged like the ſhell in the laſt experiment, was burſt into numerous fragments by $\frac{1}{4}$ of an ounce avoirdupois of the mercurial powder. The fragments were projected with but little force, and only to the diſtance of eight or ten yards. The charge of gunpowder required for grenades of this ſize, is three ounces.

Exp. 7. A ſea grenade, of the ſame diameter as the laſt mentioned, and charged in the like manner, with $\frac{1}{8}$ of an ounce avoirdupois, or 57$\frac{1}{2}$ grains, of the mercurial powder, was ſplit into two equal pieces, which were not thrown ten inches aſunder.

The report in the four laſt experiments was very ſharp, but not loud in proportion.

It ſeems, from the manner in which the ſwivel was burſt, in the fourth experiment, that a ſmaller charge would have been ſufficient for the purpoſe. We may therefore infer,

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both from this inſtance and from the ſecond experiment made with the gun, in Section V, that any piece of ordnance might be deſtroyed by employing a quantity of the mercurial powder equal in weight to one-half of the ſervice charge of gunpowder; and, from the ſeventh and laſt experiment, we may alſo conclude that it would be poſſible ſo to proportion the charge of mercurial powder to the ſize of different cannons, as to burſt them without diſperſing any ſplinters. But the great danger attending the uſe of fulminating mercury, on account of the facility with which it explodes, will probably prevent its being employed for that purpoſe.

In addition to the other ſingular properties of the fulminating mercury, it may be obſerved, that two ounces inflamed in the open air, ſeem to produce a report much louder than when the ſame quantity is exploded in a gun capable of reſiſting its action. Mr. Cruickſhank, who made ſome of the powder by my proceſs, remarked that it would not inflame gunpowder. In conſequence of which, we ſpread a mixture of coarſe and fine grained gunpowder upon a parcel of the mercurial powder; and, after the inflammation of the latter, we collected moſt, if not all, of the grains of gunpowder. Can this extraordinary fact be explained by the rapidity of the combuſtion of fulminating mercury? or is it to be ſuppoſed, (as gunpowder will not explode at the temperature at which mercury is thrown into vapour,) that ſufficient caloric is not extricated during this combuſtion?

From the late opportunity I have had of converſing with Mr. Cruickſhank, I find that he has made many accurate experiments on gunpowder; and he has permitted me to ſtate, "that the matter which remains after the exploſion of gunpowder, conſiſts of potaſh united with a ſmall proportion of carbonic acid, ſulphat of potaſh, a very ſmall quantity of ſulphuret of potaſh, and unconſumed charcoal. That 100 grains of good gunpowder yield about 53 grains of this reſiduum, of which three are charcoal. That it is extremely deliqueſcent, and when expoſed to the air, ſoon abſorbs moiſture ſufficient to diſſolve a part of the alkali; in conſequence of which, the charcoal becomes expoſed, and the whole aſſumes a black, or very dark colour." Mr. Cruickſhank likewiſe informs me, that after the combuſtion of good gunpowder under mercury, no water is ever perceptible.

References to the Figures of the Glaſs Globe, &c. mentioned in Section VII. (See Plate VIII.)

A, a ball or globe of glaſs, nearly half an inch thick, and ſeven inches in diameter. It has two necks, on which are cemented the braſs caps B, C, each being perforated with a female ſcrew to receive the male ones D, E: through the former a ſmall hole is drilled; the latter is furniſhed with a perforated ſtud or ſhank G. By means of a leather collar H, the neck C can be air-tightly cloſed. When a portion of the powder is to be exploded, it muſt be placed on a piece of paper, and a ſmall wire laid acroſs the paper, through the midſt of the powder: the paper being then cloſed, is to be tied at each end to the wire, with a ſilken thread, as ſhown at I. One end of this wire is to be faſtened to the end of the ſhank G, and the ſcrew D inſerted to half its length into the braſs cap B; the other end of the wire, *a*, by means of the needle K, is to be drawn through the hole F. The ſcrew E being now fixed in its place, and the wire drawn tight, it is to be ſecured by puſhing the irregular wooden plug L into the aperture of the ſcrew D, taking care to leave a paſſage for air. The ſtop-cock M, the ſection of which is ſhown at N, is now to be ſcrewed on to the part D, which is made air-tight by the leather collar *b*. The glaſs tube O is bent, that it may more conveniently be introduced under the receiver of a pneumatic apparatus. P ſhows the manner of connecting the glaſs tube with the ſtock-cock.

VI. *Comparative Analyſis of Human Bones, and thoſe of different Animals.* *By* C. Merat-Guillot, *Apothecary at Auxerre**.

BUFFON ſays, "We ought to collect objects of every kind, to compare them, to ſtudy their nature, and from their combined relations to deduce that information which may enable us to acquire a clear conception of them, and to increaſe our knowledge reſpecting them*." Fourcroy obſerves, in his Elements of Chemiſtry, under the head Bone, that "We cannot refuſe believing that the bones of man and of quadrupeds are of a different nature from the ſoft and flexible bones of fiſhes, reptiles, and, above all, the corneous ſkeletons of inſects, as well as from the calcareous teſtaceous covering of worms which have ſhells."

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* From the *Annales de Chimie*, No. 100.

Being deſirous to aſcertain the truth of what had been advanced by C. Fourcroy, and guided by what is preſcribed by Buffon in the treatiſe above mentioned, I undertook a labour, ſuperior, no doubt, to my ability, and in which it is poſſible I may have been deceived, but which I ſubmit to the judgment of thoſe who are maſters in the art. I was deſirous of procuring all the objects which it was neceſſary ſhould be ſubjected to compariſon, ſuch as a ſeries of human bones, from which I might trace out the progreſs of oſſification; but as I could not, and wiſhing to know at leaſt the difference between products of animal bones at an early age, and thoſe of the ſame animal at an advanced age, I employed for that purpoſe the bones of oxen and calves. I wanted, to complete my experiments in this analyſis, the bones of carnivorous animals, the ſkeletons of inſects, &c. When I am able to procure them, I ſhall reſume my labours.

Being deſirous to comprehend in this analyſis, hair, corneous ſubſtances, and the briſtles of different animals, I remarked, when treating the firſt of theſe ſubſtances with cauſtic ſoda, by means of heat, that there was produced a conſiderable diſengagement of ammonia; that in pouring into this ſolution the muriatic acid, there was diſengaged ſulphurated hydrogen, and that a blackiſh precipitate, but in very ſmall quantity, was thrown down. Having boiled this liquor, there was ſtill diſengaged a great deal of ſulphurated hydrogen, from which I conjectured that it contained ſulphur; and this fact I wiſhed to aſcertain. With that view, I put a ſilver ſpoon into the liquid, and again boiled it; the ſpoon became exceedingly black, and I thence concluded that the hair contained ſulphur. As this labour preſented peculiar reſults, which cannot be compared with the analyſis here given, I intend, having already collected notes on that ſubject, to make a particular analyſis of this ſubſtance compared with that of other ſubſtances of an analogous nature.

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* *Diſcours de la Manière d'Etudier et de Traiter l'Hiſtoire Naturelle*, p. 16.

It is to be obſerved that I always operated on 100 parts of each ſubſtance, and that all the products, the quantities of which I determined, had been previouſly dried as much as poſſible.

Names of the Subſtances.	Proportions of Gelatinous Matter.	Proportions of the Phoſphat of Lime.	Proportions of the Carbonat of Lime.	Loſs.
Human bones taken from a burying-ground	16	67	1·5	15·5
Dry human bones which had not been in the earth	23	63	2	2
Bones of an ox -	3	93	2	2
——- of a calf -	25	54	traces of it	21
——- of a horſe	9	67·5	1·25	22·25
Teeth of a horſe	12	85·5	0·25	2·25
——- of an elephant, or ivory	24	64	0·1	11·15
Bones of a ſheep	16	70	0·5	13·5
——- of the elk.	1·5	90	1	7·5
Horns of the ſtag	27	57·5	1	14·5
Bones of a ſwine	17	52	1	30
——- of the hare	9	85	1	5
——- of a hen -	6	72	1·5	20·5
Egg-ſhells - -	3	2	72	23
Bones of the pike	12	64	1	23
——- of the carp	6	45	0·5	48·5
——- of the viper	21·5	60·5	0·5	17·5
Lobſter ſhells - -	18	14	40	28
Mother-of-pearl	2·5	0	66	31·5
Crab's eyes - -	2	12	60	26
White coral - -	1·5	0	50	48·5
Red ditto - -	0·5	0	53·5	46
Articulated coralline - - -	7·5*	0	49	43·5
Cuttle-fiſh bones	8	0	68	24

Being deſirous to aſcertain whence aroſe the loſs experienced in each analyſis, I ſubjected to a ſtrong heat 100 parts of

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[*] The product which I place under the head of Gelatinous Matter, is not entirely compoſed of that ſubſtance. In my opinion it contains a portion of a ſubſtance analogous in its nature to that which forms the polypiers known under the name of *lithophytes*.

of crabs eyes: they decreaſed 22 parts, and for that reaſon, I preſume that this loſs might be aſcribed to the water which is found, at leaſt in the gelatinous matter, ſince it is dried as much as poſſible; but as there ſtill remains four parts, I aſcribe the loſs to a portion of the gelatinous matter which is found diſſolved in the liquid employed for the analyſis, and to a ſmall quantity of a ſaline ſubſtance which I did not examine, and which may be diſſolved in it. Should this eſſay be of any utility, the end I propoſed will be anſwered [*].

VII. *Method of determining the Quantity of Sulphur and Iron contained in Yellow Copper Ore. By* B. G. SAGE, *Director of the firſt School of Mines* [†].

THE colour of this ore is a bright yellow inclining to that of gold; on the ſurface of it there is often obſerved reddiſh, violet, purple, and greeniſh blue varying ſhades, on which account it has been called peacock's-tail or pigeon's-neck copper ore. The form of this yellow copper ore is the equilateral tetraedron and its varieties. Though it is rare to find theſe cryſtals regular, the ore, in a compact, irregular, and ſometimes lamellated maſs, is very common; ſome of it is met with in the ſchiſtous impreſſions of fiſh found at Eiſleben and Mansfeld.

I have ſaid, in the ſecond volume of my Mineralogy, that the yellow copper ore always contains iron and ſulphur in greater or leſs quantity: the proportions I have been able to determine by the following experiments. The yellow copper ore which I employed had no matrix. Having pulveriſed it in a copper mortar, four hundred grains of it were expoſed to torrefaction in a teſt, but, as it was too much heated, the ore became fuſed, and produced a yellow compact brilliant maſs, which had a violet appearance in ſome parts of its fracture. This ore was not in the ſtate of ſlag, that is to ſay, in a black maſs, as is the caſe when torrefied on a large ſcale.

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[*] This analyſis agrees pretty well with that of Mr. Hatchett already given in this Magazine, Nos. 21 and 24.

[†] From the *Journal de Phyſique*, Prairial, an. 8.

Having roaſted four hundred grains of the ore, at a heat proper for keeping it red, in a teſt, four hours were required to diſengage all the ſulphur. There remained a blackiſh brown powder compoſed of calx of iron and copper. A magnet plunged into this mixed calx, came out covered with particles of iron.

The yellow copper ore ſuſtains no ſenſible diminution of its weight by torrefaction; the ſulphur, however, burns, and is exhaled in ſulphureous acid; but the metals becoming calcined, increaſe in weight by the acid and water which they abſorb: the ſulphur furniſhes acid, and the air decompoſed furniſhes water.

I determined the quantity of iron contained in the yellow copper ore, by putting a quintal of it, after being torrefied, to digeſt cold in volatile alkali. I added of it to this calx till it was no longer coloured blue: the iron remained at the bottom of the veſſel under the form of a blackiſh powder, which, when waſhed and dried, was ſuſceptible of being attracted by the magnet. This iron was equal in weight to one-half of the ore which I had put to digeſt with the volatile alkali.

I diſengaged the ſulphur from the yellow copper ore by diſtilling it with two parts of concentrated vitriolic acid: there paſſed ſulphureous acid, and at the ſame time the ſulphur was ſublimated of a citron-yellow colour, and condenſed in the neck of the retort. When detached, waſhed, and weighed, it amounted to a fifth part of the ore. This proportion is the ſame as that of the ſulphur, which ſerves to mineraliſe antimony, biſmuth, and cobalt.

The reſiduum of the diſtillation of the yellow copper ore and vitriolic acid having been waſhed, filtered, and evaporated, produced in the firſt inſtance martial vitriol (or ſulphat of iron) in beautiful rhombs; it had a blueiſh tint, ariſing from the copper.

The yellow copper ore may be decompoſed by the nitric acid at twenty-ſix degrees, which diſſolves with effervеſcence the copper and the iron, and aſſumes a beautiful green colour. Nitrous acid muſt be repeatedly digeſted over the copper ore till it no longer become coloured, and until the ſulphur remain at the bottom of the matraſs under the form of a white powder, which ſometimes retains a little iron.

Having reduced the yellow copper ore with three parts of black flux and a fifth of charcoal, it produced *per* quintal thirty pounds of friable copper, in part ſubject to be attracted by the magnet. It appears by this reduction that the ore yields *per* quintal,

Copper - -	40
Iron - -	40
Sulphur - -	20

It is the fixed alkali of the black flux which diſſolves the copper; ſo that if the reduction of this metal were attempted without adding charcoal, there would be danger of not obtaining the button.

VIII. *Method of determining the Quantity of Acid of Sugar contained in Spirit of Wine.* *By* B. G. SAGE, *Director of the firſt School of Mines* *.

URBAN HIERNE, a Swediſh chemiſt, firſt ſhowed, that when ſpirit of wine is decompoſed by nitrous acid, a ſalt is obtained as reſiduum on cooling. It was reſerved, however, for Scheele to make known that this ſalt is of the ſame nature as the acid of ſugar †.

I long conſidered ſpirit of wine ‡ as an acid ſoap, ſince the ether and the oil of wine which compoſe it, are miſcible with water. The acid of ſugar is the medium of union, and is found in it in a pretty conſiderable proportion. I was not able, however, to determine it till after a great number and variety of experiments. That which anſwered the purpoſe beſt was as follows:—I put into a glaſs retort one part of ſpirit of wine, and three parts of nitrous acid at thirty-two degrees, adapted the retort to a ſeries of balloons, which I did not lute, and expoſed to a ſmall degree of heat the ſand-bath in which I had placed the retort. At about the thirtieth degree of Reaumur, the mixture was decompoſed; an efferveſcence, accompanied with large bubbles and red vapours of nitrous gas was produced; and at the ſame time the ether paſſed with an exploſion into the receiver, and perfumed the laboratory. It is neceſſary in this experiment that the retort ſhould be ſufficiently large to contain twelve times the quantity of the mixture introduced into it.

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* From the *Journal de Phyſique*, Prairial, an. 8.

† The acid of ſugar is known in the new nomenclature under the name of *oxalic acid*, but why not ſpeak French? Why not leave it the name of acid of ſugar, ſince it is in that ſalt that this acid is moſt abundant, as ſixteen parts of ſugar produce ten parts of concrete acid?

‡ Rectified ſpirit of wine is called, but improperly, by the neologiſt chemiſts, *alcohol*. This term, ſays the dictionary of Trevoux, is derived from the Arabic word *kol*, which ſignifies to render ſubtle, to diminiſh, to reduce to an impalpable powder. It cannot therefore be applicable but to ſolid bodies. Philoſophers ought to employ in their language more preciſion and correctneſs.

The ether being thus diſengaged, the fire was kept up under the ſand-bath, and the diſtillation was continued, until no more than about a thirty-ſecond part of the mixture remained. The ſand-bath was then ſuffered to cool, and I found at the bottom of the retort beautiful cryſtals of the acid of ſugar, in tetraedral priſms, under a little water ſlightly acid.

From ſixteen ounces of rectified ſpirit of wine, I obtained one ounce, one dram, and twenty-four grains of concrete acid of ſugar. When it is in combination with ether and the eſſential oil of wine, it neutraliſes their odour as well as the oils which render it volatile. What appears remarkable is, that it burns with them, ſince the combuſtion of pure ſpirit of wine, which takes place in the open air, leaves nothing.

Though the boiling heat is neceſſary for diſengaging from wine the inflammable ſpirit, it nevertheleſs exiſts in it. In my opinion it is engaged there by a portion of tartar which is found in the reſiduum of the diſtillation of the wine. The heat acts alſo, in all probability, on a glutinous matter ſimilar to that found in the dregs of wine: after it is ſeparated and inſpiſſated, the ſpirit of wine is diſengaged and volatiliſed.

When a mixture of equal parts of ſpirit of wine and concentrated vitriolic acid is diſtilled, the eſſential oil of the ſpirit of wine is reſiniſied and charred, the ether is liberated from its confinement. This kind of eſſential oil, ſurcharged with inflammable matter, is inalterable by acids, and may be conſidered as inflammable gas in the form of a fluid, of which it has indeed all the properties.

IX. *Comparative View of ſome dangerous Diſeaſes, ſuppoſed to be occaſioned by Inſects, which prevail in Sweden, Ruſſia, Siberia, and the adjacent Countries* *.

DR. SOLANDER was the firſt perſon who obſerved, with the eye of a philoſopher, a diſeaſe known in Weſt Bothnia and the neighbouring parts of Lapland under the name of *ſkät* or *ſkütz* †. This diſeaſe attacks thoſe only who travel or labour in the open air; and in the above diſtricts it is conſidered as the effect of the witchcraft of the Laplanders. The common people believe that the Laplanders cauſe balls of the hair of their cattle to fly through the air, and that the hairs which fall from theſe balls penetrate the bodies of men and animals, according to the pleaſure of the witch or ſorcerer, and occaſion their death with excruciating pain.

Except in Eaſt and Weſt Bothnia, Torneo, Kemi, and other parts on the gulph of Bothnia, this diſeaſe is exceedingly rare in Sweden. In thoſe parts, however, it is generally known, and attacks ſome of the inhabitants every year. Linnæus conjectured that this diſeaſe was occaſioned by a ſmall kind of worm, one of which, in a dried ſtate, had been ſent to him from Kemi ‡; but he concluded, very erroneouſly, that the diſeaſe might be of the ſame nature as that prevalent among the horſes in Holland, and known under the name of the *vyvel* or *feyſel*. This is now proved to be a miſtake. Dr. Solander, however, has placed it beyond all doubt, that a ſmall worm which penetrates into the fleſh of living animals, is the occaſion of thoſe dangerous and mortal tumours with which healthy people are often unexpectedly attacked in the before-mentioned diſtricts. According to his obſervations, the extenſive fens in the northern part of Bothnia are the principal ſeat of this deſtructive inſect, and the tumours it produces have been remarked chiefly by the inhabitants of theſe diſtricts or thoſe who travel through them. In men, this diſeaſe generally attacks thoſe parts of the body which are expoſed to the air, ſuch as the hands, arms, neck, &c. A prick like that of a needle is firſt felt, after which a black ſpeck is ſeen in the place; and a violent itching enſues. This is ſoon after followed with extreme pain, a red ſpot is produced, and the neighbouring parts are inflamed. As the diſeaſe increaſes, the parts become more irritated and painful, and the patients are ſeized with an inflammatory fever, accompanied with fits of fainting and delirium, which often carries them off in the courſe of a few days, and ſometimes of a few hours. Some, however, get over this fever, but are affected in the injured part with malignant and tedious ſores, which often remain incurable during their whole lives.

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* From *Neue Nordiſche Beyträge*, by Profeſſor Pallas, Vol. I.

† *Furia infernalis*, Dan. Chr. Solander in *Nov. Acta Upſalienſia*, Vol. I. p. 44.

‡ *Amœnitates Academicæ*, Vol. III. p. 322.

Dr. Solander was himſelf an eye-witneſs of a caſe of this kind, and he mentions others which were related to him by perſons worthy of credit. The greater part of them occurred in the ſpring and ſummer, though inſtances of this diſeaſe have been obſerved in winter. When the worm has penetrated into the fleſh, it is often impoſſible to ſave the patient even by cutting out the affected part; but it ſometimes happens that the ſmall animal which is the cauſe of the evil may be extracted with a pair of pincers, or the teeth, if the proper means be employed immediately after the puncture is felt. When this is done by cutting, the patient, if the worm has penetrated to a great depth, is expoſed to the moſt excruciating pain. Old women, who perform the operation of cutting the fleſh or extracting the worm with a variety of ſuperſtitious practices, are accuſtomed to apply birch-tar to the wound, and then to bind it up. In one caſe, the worm was extracted by the accidental application of curdled milk or new cheeſe, and the patient cured in the courſe of ſix hours. This remedy was afterwards repeated with ſucceſs. When the patient dies, the deſtructive inſect, it is ſaid, comes forth of its own accord from the mortified part.

But in whatever manner it may come forth, it has always the appearance of a very tender ſmall worm, of the ſize of a hair, about half an inch in length, of a fleſh or whitiſh yellow colour, and often black at the extremities. On each ſide it is armed with a ſingle row of uncommonly tender briſtles ſtanding backwards, which enable it to penetrate the fleſh better; and in all probability theſe briſtles occaſion that acute pain which is felt, and prevent it from being extracted. No rings or indentations can be obſerved on it; and, except the briſtles, it has a great reſemblance to the hair-worm.

This is the ſingular animal which Linnæus has admitted into his ſyſtem under the very proper name of the *Furia infernalis*. It is however hardly poſſible to ſuppoſe, with this naturaliſt, that the above worm falls down from the atmoſphere; for it will be difficult to explain by what power it can raiſe itſelf into the air. But the wonder will ceaſe if we ſuppoſe that this worm may be borne aloft on bits of ſtraw or the leaves of trees, and may then be conveyed by the winds till it at length falls down on the body of ſome animal.

In different parts of the Ruſſian empire, particularly in dry ſummers, and in open marſhy diſtricts, dangerous diſeaſes of the like kind have been obſerved, but eſſentially different from that of Sweden, though in all probability produced by a ſimilar cauſe.

The elder Gmelin was the firſt perſon who remarked this diſeaſe in Siberia. He mentions it in his travels *, and gives an account of its ſymptoms as well as of the remedies employed to cure it. In order that the effects of the Ruſſian diſeaſe may be compared with thoſe of the Swediſh fury, I ſhall here give an account of the former according to the lateſt obſervations on the ſubject †. The open level country which extends from the Urali mountains along the river Ui, or the ſo called Ui line, the whole diſtrict of Iſchimi and Barabyni as far as it belongs to Ruſſia along the Irtiſch, and even the diſtricts between the Jaik and the Irtiſch, but particularly the low, marſhy, and ſaline fens of theſe immenſe plains which are dried up every year, and ſituations of the like kind near the rivers and brackiſh lakes from the banks of which the water retires during the droughty weather, are the places where this diſeaſe breaks out every year, and prevails more or leſs both among men and cattle. In theſe countries, the horſes and other ſmooth-haired animals never go out with ſafety in ſummer to paſture. For this reaſon the inhabitants of the banks of the Irtiſch, where this diſeaſe is moſt dangerous, drive their horſes in ſummer to the high paſtures, at a diſtance from the river, where in that ſeaſon cool winds prevail, and where this kind of plague is never obſerved to make its appearance. In the northern and eaſtern parts of Siberia, and alſo in the mountains, it is not known. This diſeaſe, which is diſtinguiſhed in the above diſtricts by the name of *morowaſa jaſwa*, or plague, is moſt common in the ſummer months, when the ſultry ſouth winds prevail. As ſoon as the winds and the weather become cold, no traces of it are to be ſeen; but if ſuch a change of temperature does not take place, the diſeaſe often continues till autumn, both among men and horſes. Smooth-haired cattle alſo, and camels, the ſkins of which in ſummer are almoſt deſtitute of hair, are ſubject it. If we now reflect, that people who reſide much in the houſe eſcape this ſcourge, and that it attacks thoſe only who are expoſed to the open air in the fields and meadows; that it chiefly affects thoſe parts which are naked or thinly covered, and that it appears moſt frequently in horſes in the belly, where the hair is thin; that ſheep, by whoſe thick wool the diſeaſe ſeems, as it were, to be checked, never ſuffer from it, and that the ſmoke of the fires which the natives kindle, protects, in ſome meaſure, their cattle; we ſhall naturally be led to conclude, when the ſymptoms alſo are taken into conſideration, that ſomething muſt penetrate imperceptibly into the body from without, and by its poiſonous quality produce inflammatory tumours and ulcers. It is probable, alſo, that it is ſomething endowed with life, which is conveyed through the air to the marſhy plains, where it attacks animals, and forces its way into their fleſh. But whatever

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* Part IV. p. 143.

† Pallas *Reiſe durch verſchiedene provinzen des Ruſſiſchen reichs*, Part II. p. 308.

ever it may be, the nature of it has not yet been determined by certain obſervation. The puncture of flies and gnats, eſpecially when they have fed upon carrion, is in thoſe diſtricts of a poiſonous and inflammatory nature: but theſe inſects are too perceptible, and their effects ſo ſenſible, that we cannot aſcribe to them a diſeaſe which at firſt is not attended with any remarkable pain, and in this reſpect it is particularly diſtinguiſhed from the Swediſh.

The ſymptoms of this diſeaſe, as generally obſerved, are as follows:—Perſons in perfect good health, of all ages and of both ſexes, unexpectedly feel, while walking in the fields, an itching accompanied with a ſmall hard tumour, which at firſt they are inclined to conſider as the effect of the puncture of a gnat or fly. It, however, ſpeedily increaſes in ſize and hardneſs, and before much attention has been paid to it, has proceeded to ſuch length, that a needle may be ſtuck into the hard ſwollen part without the patient being ſenſible of pain, unleſs the needle is made to reach the ſound fleſh. In the centre of the tumour there is then ſeen, for the moſt part, a red or blueiſh point like the prick of an inſect, and in which, if proper means be not ſoon applied, mortification or gangrene enſues, and ſpreads itſelf to the neighbouring parts. In the firſt ſtage of the diſeaſe, the patient has no internal affections; but as the inflammation of the tumour increaſes (which, however, is not attended with much pain), head-ache, dejection, and internal uneaſineſs take place: but it is not improbable that the fear of danger may contribute to promote theſe ſymptoms. Among cattle attacked by this diſeaſe, uneaſineſs and dejection are not obſerved till it has attained to the utmoſt degree of its violence.

Cattle attacked by this diſeaſe generally die, partly becauſe the tumours are obſerved too late, and partly becauſe the common people, being negligent, do not give themſelves the trouble to apply a cure; on the other hand, among the human ſpecies, a cure ſeldom fails if the proper means have been employed in time. The cure generally uſed by the common people, as mentioned in Gmelin's Travels, is, to run a long needle through the tumours in different directions, to rub into the punctures a mixture of ſal-ammoniac and tobacco, and to forbid the patients the uſe of cold liquors and of certain kinds of food. This cure, however, has been rendered ſtill ſimpler, and various other means more effectual have been deviſed. On the Irtiſch, a ſtrong lixivium of wormwood aſhes is employed, and a decoction of tobacco and ſal-ammoniac, or alum, without ſcarification. On the Ui line, a warm plaſter made of pulveriſed nightſhade, ſal-ammoniac, yeaſt, and oatmeal, is ſaid to be an infallible remedy; but the common people ſtill adhere to the old painful method of cure, which is uſed even by ſome ſurgeons. In other places the common people conſider the application of a living frog to the tumours as of ſervice; but I have no authority for warranting its good effects. The other means, as is well known by long experience, are proper for deſtroying vermin, and therefore favour the conjecture before mentioned reſpecting the primary cauſe of this diſeaſe.

In the extenſive low-lands on the Wolga, from Saratof towards Aſtracan, the cattle, and particularly horſes, when they are driven out to paſture too ſoon after the water with which the fields have been overflown has run off, frequently die in conſequence of hard inflammatory tumours which break out on the breaſt and belly, and which, without doubt, are of the ſame nature as the above-deſcribed Siberian diſeaſe. The Calmucs call theſe tumours *mohmo*, but, as appears, confound under this name other maladies, accompanied with tumours, which attack cattle. This much, however, is certain, that on the Wolga the tumours here deſcribed are prevalent among the human ſpecies; but they are moſt common in dry ſummers; and the men, who are daily expoſed to the open air, are more ſubject to them than the women, who conſtantly reſide in their ſmoky huts, or never remove to a great diſtance from them. Among the Calmucs on the Wolga, the Songari, who were acquainted with this diſeaſe in their former country, the Altai mountains, recommend, as a remedy for it, to burn on the tumour a bit of Chineſe incenſe, which conſumes in the ſame manner as tinder, and then to thruſt into the black point which appears in the middle of it, a copper or iron needle, till it penetrate to the ſound fleſh. This remedy, however, muſt be applied before the expiration of the ſecond day, elſe it will be ineffectual. The Torgateſe are accuſtomed to ſtimulate and ſcarify theſe tumours with the petrified tooth of a ſhark, which they call a ſnake's horn. I have been informed in a letter from a perſon who was an eye witneſs, that tumours of this kind in a horſe, diſſected after death, were found to conſiſt of ſoft watery matter. Horſes often die on the day when the tumours appear *. Among the Calmucs it is cuſtomary to burn all the cattle which die of the ſo called *mohmo*, but the poor are ſeldom ſo nice, they only cut out the fleſh around the tumours, and eat the remainder without any ceremony. It is ſaid that the blood of thoſe who feed on ſuch carrion becomes corrupted, and that they are ſubject to running ſores, called *chattigä*, which are often infectious. But that this obſervation is applicable rather to other diſeaſes among cattle, than to the tumours in queſtion, I conclude from this circumſtance, that the Tartars ſay, that the *mohmo* of horſes, which, as is well known, are moſt ſubject to external tumours, are not always infectious; thoſe, however, of cows and goats are more dangerous, ſo that not only the fleſh of theſe animals, when eaten inadvertently, is infectious, but even their ſkins if a perſon lies or ſits upon them. According to ſome late obſervations which have been communicated to me, purgatives adminiſtered to thoſe attacked with theſe tumours are pernicious: on the other hand, ſtrengthening ſpirituous remedies are found to be ſerviceable if uſed before the appearance of lethargic affections, which in thoſe countries ſeldom fail to accompany this diſeaſe, and which ſometimes continue two or three days. The Calmucs, as I am informed, are inclined to aſcribe the *mohmo* tumours to the poiſonous puncture or filth of an inſect, and therefore to an external cauſe.

[To be concluded in the next Number.]

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* Pallas *Sammlungen hiſtoriſcher nachrichten über die Mongoliſchen völkerſchaften*, Part I. p. 161.

X. *Deſcrip-*

X. *Deſcription of the Veſpertilio plicatus.* By FRANCIS BUCHANNAN, *M. D. A. L. S.*[*]

VESPERTILIO PLICATUS[†], tailed; the noſtrils round, ſimple perforations; the upper lip very large, and folded; the ears as large as the head, folded, and half pendulous. Inhabits old houſes at Puttahaut in Bengal.

From its teeth this bat can only be miſtaken for the *Cephalotes*; but the deſcription of that ſpecies will not apply to this.

From the point of the noſe to the root of the tail three inches; from the extremity of one wing to that of the other twelve inches.

The wings, and naked parts of the body, are ſoot-coloured. The hair is mixed with aſh-colour, and is paler below than on the back.

The head is large, thick at the ſhoulders, and tapers gradually to the ſnout; which is blunt, terminates in a heart-ſhaped margin, and projects far beyond the lower jaw. It is moſtly naked; but has ſeveral long, erect briſtles. The noſtrils are ſmall circular holes, remote from each other, and placed under the margin of the ſnout. The upper lip at the ſides hangs over the under jaw, and at each ſide is deeply wrinkled with ſeven or eight vertical folds. The ears are large, blunt, wrinkled, and ſomewhat pendulous. From being bent in ſeveral folds, they at firſt ſight appear to be thick and fleſhy. They approach very near at their inſertion on the brow, and are naked, except on a ſharp ſinus towards the hinder part of the head. On their edge near the tips are five or ſix ſmall warts. There are no internal auricles. The eyes are in two ſmall ſlits above the angles of the mouth, and are almoſt covered by the ears. There are two ſtrong tuſks in each jaw. In the upper jaw there are two conical ſharp fore-teeth, half as long as the tuſks. Below, in place of theſe, there are only two ſmall points, ſcarcely projecting from the gums. The grinders are a little removed from the tuſks, and are in each ſide of each jaw five or ſix in number. In the lower jaw each grinder has two ſharp points; in the upper jaw each, except the firſt pair, has three points.

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[*] From the *Tranſactions of the Linnæan Society*, Vol. V.

[†] Section F of Ker's Tranſlation of Gmelin's Syſt. Nat.

The neck is very ſhort, and ſo covered with hair as to be ſcarcely obſervable. The ſhoulders are high and round, with a deep cavity between them. The body at the ſhoulders is much wider than at the haunches. The buttocks are bare.

The tail is naked, round, and blunt: it is turned up at the end. A ſtrong hooked claw in place of the thumb. No carpus nor metacarpus. Four long fingers ſerve to diſtend the membrane of the wing. The hind-feet have five diſtinct toes, with ſmall ſharp claws. The membrane of the wings joins the hinder legs and tail, but it is not nearly ſo long as the latter. A broad hairy liſt ſurrounds all round the body, and covers the bottom of the membrane.

The drawing and deſcription were taken from a male. I could obſerve no nipples.

The natives of Bengal have only two names for all the ſpecies of bats found in their country. The large bats, which neſtle on trees, and live chiefly on fruit, they call *Badur:* thoſe which, like the one above deſcribed, inhabit caverns and old buildings, and live chiefly on inſects, they call *Chamcheeka.*

Plate IV. repreſents the *Veſpertilio plicatus* of nearly one-half its natural ſize.

XI. *Account of a Cavern diſcovered on the North-Weſt Side of the Mendip Hills, in Somerſetſhire.* By GEORGE SMITH GIBBES, *M. B. F. L. S.**

PERHAPS the following account of a cavern which I viſited ſome time ſince may be acceptable, as we there ſee the proceſs going on, which Nature employs to encloſe foreign ſubſtances in the harder rocks.

At the bottom of a deep ravine on the north-weſt ſide of the Mendip hills, in Somerſetſhire, near the little village of Berrington, there has been diſcovered a cavern of conſiderable extent, in which was found a great collection of human bones.

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* From the *Tranſactions of the Linnæan Society,* Vol. V.

As I have obſerved in this cavern many circumſtances which appear curious to me, I beg leave to mention them, as I do not believe there is another place in the kingdom where the different ſtages (if I may be allowed the expreſſion) of bones incorporating with limeſtone rocks can be ſo well ſeen. From the top and ſides there is a continual dripping of water, which being loaded with a large quantity of calcareous earth, depoſits a white kind of paſte on moſt parts of the cavern. Many of the bones are incruſted with this cement, and a large proportion of them are actually fixed in the ſolid rock. I ſuppoſe, therefore, that this ſubſtance, which at firſt is in a ſtate reſembling mortar, by loſing its water, hardens into a firm and ſolid ſtone. I had an opportunity of examining the proceſs in every part. Had the cavern not been diſcovered, and theſe depoſited ſubſtances not been removed, I do not doubt that the whole excavation would, in no great length of time, have been completely filled up. The water was ſtill bringing freſh quantities of calcareous earth, and the bones were in ſome places completely incorporated with the ſolid rock. Every degree of intermediate ſolidity was plainly diſcernible. There were ſeveral nodules of ſtone, each of which contained a perfect human ſkull. The ſubſtance which is depoſited from the water effervesces with acids, and has, in ſhort, every character of limeſtone. At the further end of this very curious cavern, where the height is about fifteen feet, there depends a moſt beautiful ſtalactite, perfectly conical, which, when the cavern was firſt diſcovered, reached within an inch of a cone of the ſame kind which riſes from the floor. By ſome accident a ſmall part of the ſtalactite was broken off; but Nature is now buſy in repairing an injury which had been done to one of the prettieſt productions of her mineral kingdom. Had theſe two cones met, a beautiful column would have been formed of nearly fifteen feet in height. On ſtriking this ſtalactite, a ſound is produced ſimilar to that of a bell, which may be heard at a conſiderable diſtance beyond the mouth of the cavern.

I examined the bones with conſiderable attention, and I found that there was adhering to the ſurface of many of them, a ſubſtance which reſembled the ſpermaceti I have before deſcribed in the Philoſophical Tranſactions for the years 1794 and 1795.

I have to add, that this cavern was diſcovered about two years ago by accident, and that no ſatisfactory reaſon has been given for this ſingular accumulation of human bones.

XI. *Deſcription of a Mercurial Air-holder, ſuggeſted by an Inſpection of Mr. Watt's Machine for containing Factitious Airs.* By WILLIAM CLAYFIELD *.

SEVERAL modes of counteracting the preſſure of a decreaſing column of mercury having been thought of, in conjunction with Mr. W. Cox, the following was at laſt adopted as the moſt ſimple and effectual:

Plate V. Fig. 1. repreſents a ſection of the machine, which conſiſts of a ſtrong glaſs cylinder A, cemented to one of the ſame kind B, fitted to the ſolid block C, into which the glaſs tube D is cemented for conveying air into the moveable receiver E.

The braſs axis F, fig. 2, having a double bearing at *a*, *a*, is terminated at one end by the wheel G, the circumference of which is equal to the depth of the receiver, ſo that it may be drawn to the ſurface of the mercury, by the cord *b*, in one revolution; to the other end is fitted the wheel H, over which the balance-cord *c*, runs in an oppoſite direction in the ſpiral groove *e*: a front view of the wheel H is ſhown at Fig. 3.

Having loaded the receiver with the weight I, ſomething heavier than may be neceſſary to force it through the mercury;

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* From an ingenious work lately publiſhed entitled, *Reſearches Chemical and Philoſophical, chiefly concerning nitrous Oxyd or dephlogiſticated nitrous Air, and its Reſpiration*; *by Humphrey Davy, Superintendent of the Medical Pneumatic Inſtitution.*—This apparatus was firſt deſcribed in the Third Part of Dr. Beddoes's Conſiderations, but it has ſince received ſome improvements.

cury, it is balanced by the ſmall weight K, which hangs from that part of the ſpiral where the radius is equal to that of the wheel G; from this point the radius of the ſpiral muſt be increaſed in ſuch proportion, that in every part of its circuit, the weight K may be an exact counterpoiſe to the air-holder. In this way, ſo little friction will be produced, that merely plunging the lower orifice of the tube D under mercury contained in the ſmall veſſel L, will be ſufficient to overcome every reſiſtance, and to force the gas diſcharged from the beak of a retort into the receiver, where, whatever may be its quantity, it will be ſubjected to a preſſure exactly correſponding to that of the atmoſphere. The edge of the wheel H being graduated, the balance-cord *c* may be made to indicate its volume.

Should it at any time be neceſſary to reduce the preſſure to the medium ſtandard of the barometer, it may eaſily be done by graduating the lower end of the tube D, and adding to the weights I or K, as may be found neceſſary; the ſurface of the mercury in the tube pointing out the increaſe or diminution.

The concavity at the top of the internal cylinder is intended to contain any liquid it may be thought proper to expoſe to the action of the gas.

The upper orifice *f*, with its ground-ſtopper, is particularly uſeful in conveying air from the retort *g*, with its curved neck, into the receiver, without its paſſing through the tube D. In all caſes where a rapid extrication of gas is expected, the retort *g* ſhould be firmly luted to the orifice, and the weight I removed from the top of the receiver; this, by diminiſhing the preſſure, will admit the gas to expand freely in the air-holder at the inſtant of its formation, and prevent an exploſion of the veſſels. The ſame caution muſt be obſerved whenever any inflammation of gas is produced by the electric ſpark.

The air may be readily transferred through water, or even mercury, by the tube *h*, Fig. 1.

To prevent an abſorption of mercury in caſe of a condenſation taking place in the retort made uſe of for generating air, Mr. Davy has applied the ſtop-cock *i*, to which the neck is firmly luted. This ſtop-cock is likewiſe of great ſervice in ſaturating water with acid or alkaline gaſes, which may be effected by luting one end of the tube *k* to the ſtop-cock and plunging the other into the fluid in the ſmall veſſel *l*, cemented at top, and terminating in the bent funnel *m*; the tube *b* having been previouſly removed, and the lower orifice of the tube D either ſunk to a conſiderable depth in mercury, or cloſed with a ground-ſtopper. The bend of the funnel *m* may be accurately cloſed by the introduction of a few lines of mercury.

The application of the ſtop-cock *n*, has enabled Mr. Davy to perform ſome experiments on reſpiration with conſiderable accuracy.

XII. *Curſory View of ſome of the late Diſcoveries in Science.*

[Continued from Page 80.]

FOSSILS.

G. A. DELUC has examined the lenticular numiſmal, and the belemnite. He conſiders the lenticular as the bones of a kind of ſepia. Naturaliſts in general conſider it as a kind of cornu ammonis.

He has the ſame opinion reſpecting the belemnite. "It is extremely probable," ſays he, "that this foſſil has been the ſoft bone of a fiſh."

The numiſmal is found in Europe, in Egypt, in India near the Ganges, and in Bengal.

He found at Mount-Salève a petrified buccinum ſimilar to thoſe found at Ermenonville.

He obſerved phollades in the columns of the temple of Serapis near Pozzuolo: they are in a part of the column, raiſed at preſent twenty-ſeven feet above the level of the waters of the ſea. He ſuppoſes that this temple has been thrown into the ſea by an earthquake; that the phollades then penetrated into the columns; and that a new earthquake conveyed them back to the place where they now ſtand.

FAUJAS has begun to give a deſcription of the foſſils found in the mountain of Saint-Pierre, near Maeſtricht. They conſiſt of different ſhells, and a great number of bones. Whole jaw-bones, more than four feet in length, have been found in that mountain. They are ſuppoſed to have belonged to crocodiles. He has deſcribed alſo ſome ſhells which appear to have belonged to the common tortoiſe, the *tortue franche* of Lacepede, or the *teſtudo mydas* of Linnæus. This further confirms, that the remains of animals ſtill exiſting are found among foſſils.

Denis Montfort has publiſhed a memoir on a particular foſſil kind of the cornu ammonis, found in the environs of Rouen and in other places. It is not of a ſpiral form like the common cornua ammonis, but ſhaped like a buccinum.

Villars has found foſſil wood on the mountain of Laus, in the canton of Piſans, at the height of 2537 feet, that is to to ſay, 932 feet above the moſt elevated line at which trees grow at preſent. The trees of which foſſil remains are found, are the trembling poplar, birch, and common larch.

GEOLOGY.

Bertrand has given a new explanation of his geologic ſyſtem. He ſtill ſuppoſes that the globe of the earth was originally a frozen maſs, and that along with motion it received light, liquidity, heat, and life.

Jens Esmarck, a learned mineralogiſt, has travelled through the Bannat, Tranſylvania, and Hungary, countries exceedingly rich in metallic veins. He has deſcribed theſe veins with accuracy, and explained the formation of them according to the theory of Werner, who, as is well known, thinks that ſuch veins were originally fiſſures, afterwards filled up with minerals. But ſome facts related by the author ſeem to ſhow that this ſyſtem cannot be maintained, for at Chemnitz there are veins of from 100 to 114 feet in thickneſs: at Kremnitz, the principal vein is 656 feet in thickneſs. The author endeavours to ſhow that theſe veins ariſe from the union of more than twenty ſmaller veins divided by partitions of the rock.

Humboldt, by the voyage which he has undertaken round the globe, will greatly enlarge our geological knowledge, becauſe he generaliſes his obſervations. The following are thoſe which he has tranſmitted from Cumana reſpecting the famous Peak de Teyde, called commonly the Peak of Teneriff: —“ The Peak of Teyde, which riſes to the height of 1904 toiſes, is an enormous baſaltic mountain that ſeems to reſt on denſe and ſecondary calcareous ſtone. The latter is of the ſame kind as that found with a great deal of flint at Cape Non in Africa, at Cadiz, in the Channel, in Provence, and on which the baſaltes of Saint-Loup near Agde, and that of Portugal, reſt. It is thus ſeen with what uniformity the globe is conſtructed. The Azores, the Canaries, and the Cape Verd iſles, appear to be a continuation of the baſaltic rocks of Liſbon.”

“ The waves thus carry with them, and caſt on ſhore in Africa and Teneriff, granites, ſyenites, and granitic micaceous ſchiſt, which I have ſeen at Saint-Gothard. It is to be ſuppoſed that the high ridge of Mount Atlas, which extends from the Eaſt towards the coaſts of Morocco, conſiſts of ſuch rocks.”

“ The baſaltes of which the Peak is conſtructed, is not only baſaltes containing foliaceous and cryſtalliſed olivin and volcanic chryſolite, but particularly towards the top there are ſtrata of the *porphyroſchiefer* of Werner, or of another kind of porphyry with an obſiadian baſe. The *porphyroſchiefer* is foliated, ſonorous, ſemi-tranſparent at the edges, has a very hard green baſe, and contains cryſtals of vitreous feld-ſpar.

[To be continued.]

XIII. *Obſervations on Ants, and on the Poiſon of theſe Inſects; with ſome Hints for deſtroying them.* By M. Amoureux *jun. M. D.*[*]

THE provident ant, which has been ſo often quoted as an example of foreſight and economy, does not, as has been believed, ſtore up proviſion; it is only laborious, and collects, indiſcriminately, things that are eatable and others that are not; the former to ſupply it with food for the moment, and the latter to be employed as materials neceſſary for ſupporting ſubterraneous galleries and for giving occupation to labourers; for their ſociety, like that of the bees, conſiſts of males and females, and a neutral ſpecies, or individuals without ſex. It is the latter that are charged with the internal labour, and the care of nouriſhing the young. The labouring ants are diſtinguiſhed from the reſt by being deſtitute of wings; they enjoy alſo a longer life than the males or the females, and they often remain alone with the larvæ in the neſt. Ants continue in a ſtate of torpor during the winter; and it may thence be preſumed that the articles they ſtore up do not conſiſt of food which they wiſh to provide to afford them a ſupply during periods of ſcarcity. Their granaries are not filled with proviſions, ſince they ſtore up, along with grains of corn, bits of ſtraw and all other ſmall articles which they can find. Naturaliſts, therefore, have been miſtaken reſpecting this pretended foreſight, by aſcribing to a laborious inſect a manœuvre of which man alone is capable.

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[*] From *Notice des Inſectes de la France reputés venimeux.*

The ant is not accounted venomous, yet it is more ſo than moſt other inſects. There ariſes from its body an acid vapour which is not perceptible but when they are collected in a large number, as in an ant-hill, or when they are heaped up in a box or bottle, &c. For this reaſon ants leave traces behind them, by a ſort of ſcorched appearance, on the graſs and plants over which they paſs and repaſs in going backwards and forwards from their neſts. Gardeners, therefore, have great reaſon to wiſh for the diſcovery of ſome certain means that might free them from theſe troubleſome gueſts, which ſome have ſuppoſed to be uſeful by having ſeen them attack the ſmall inſects which injure the leaves of fruit-trees, and particularly thoſe of the peach. Ants, indeed, are attracted by the melleous liquor ſhed by ſome of theſe inſects, and which moiſtens their bodies, but they do not on that account carry on war againſt them; in concert with them they deſtroy the plants and trees, on which they aſſemble in large bodies.

The ant pinches very ſtrongly with its mouth, which is armed with jaws; it punctures alſo by means of a ſting at its extremity, which is wanting in the males: it can do injury, therefore, three different ways. Swammerdam ſays that the jaws or forceps of the ant have on each ſide ſeven points or ſmall teeth; and Leuenhoek pretends that it emits from its ſting ſmall drops of a certain liquor which occaſions pain, as is the caſe with that of the ſcorpion. Probably it is nothing elſe than its acid liquor which is cauſtic.

This inſect is very extraordinary, not only on account of its form, but on account of its ſmall ſize; and being ſo commonly ſeen, it has not been examined with ſufficient attention. Thoſe who wiſh to ſee ants of a gigantic appearance, may inſpect the figure given of one by Gundelius Ab Ach, who flattered himſelf that, in 1687, he had invented a microſcope that magnified twice as much as any ever before conſtructed. The ſame author has given the figure of a winged ant of the dimenſions of eleven inches.

We may conſider ourſelves happy, that among the ſmall number of ſpecies of this inſect known in France, which Mr. Geoffroy makes to be only ſix, there are none of thoſe voracious and deſtructive kinds ſo troubleſome in the burning climates of Africa, in Egypt, and in South America. A ſwarm of ants which have lodged in a hollow tree, iſſue from it on its being in the leaſt ſhaken, and, like a ſhower of fire, fall on the unhappy perſon expoſed to their fury. One of our moſt learned naturaliſts, going along the banks of the Niger, found himſelf inveſted by a ſort of red ants which lodge in the icaco-tree. The punctures they made were ſo venomous, ſays he, that my face and hands were covered with bliſters ſimilar to thoſe ariſing from burning, the pain of which could be allayed by nothing but a heavy rain, which took place in the evening. The ſame traveller was peſtered in the iſle of Gorée by another kind of white ants (*termites*) which gnaw and deſtroy every thing that comes in their way.

Botaniſts long ago obſerved, that when the flowers of ſuccory were thrown into an ant's neſt, their blue colour was changed into red, which is, no doubt, common to the greater part of blue flowers. This phenomenon muſt at firſt have excited ſome ſurpriſe, and it was explained, no doubt, according to the phyſical knowledge of the period; but it was afterwards diſcovered that this change of colour was owing to the acid liquor which the ants had ſhed upon it. This acid ſpirit was carefully examined by ſeveral experiments, an account of which was publiſhed in the Philoſophical Tranſactions for the year 1670. M. Tiſcher appears to have been the firſt who aſcertained the nature of this acid, and this diſcovery muſt have appeared the more aſtoniſhing, as it was known that animals in general furniſhed alkali and an urinous ſpirit.

After this, chemiſts haſtened to analyſe ants and the vapours exhaled from them. Homberg extracted from them, by diſtillation, abundance of acid, and ſhowed it to the Academy of Sciences in 1712. Margraf ſoon obtained the ſame acid; but the learned Pruſſian chemiſt conſidered this acid only in regard to its relation and combinations with other chemical ſubſtances, and the ſolutions it is capable of effecting. This acid has a great reſemblance to that of radical vinegar, though it does not reſemble it in every thing: Neuman firſt perceived an odorous eſſential oil, of a peculiar ſmell, which, however, occaſions no burning ſenſation on the tongue. Margraf obtained the ſame ſubtile oil, and another fat oil ſimilar to that of vegetables. M. Thouvenel drew from ants both theſe oils, and a particular extract; a reſult of the moſt complete analyſis that ever had been made of theſe inſects.

The late M. Roux, the editor of the *Journal de Medecine*, paid attention alſo to this object, and by his curious experiments brought to light ſome facts very little known. For example, if an ant-hill be opened, and a living frog be expoſed in it, the animal will die in leſs than four or five minutes, though it has not been bit by the ants. M. Roux, being employed in collecting ants to fill a bottle, perceived in the evening that his fingers were ſomewhat hot, that they ſwelled and became red; next day the epidermis ſeparated from the ſkin as if he had applied a veſicatory, and his fingers became entirely peeled. It appears alſo, by M. Roux's experiments, that the acid vapour of ants is capable of ſuffocating them when they are ſhut up in a bottle. This may ſupply ſome hints for ſtifling their deſtructive colonies by carefully cloſing up the apertures of ant-hills and heaping earth above them. M. Roux mentions alſo an obſervation ſimilar to his own, which was communicated by Baron D'Holbach; he made at the ſame time ſome reflections on the nature of the poiſon of ants, reſpecting which he would not venture to give a deciſive opinion. This prudent modeſty in an able chemiſt may ſerve us as a leſſon.

Meſſrs. Ardwiſſon and Oerhne have alſo made a number of curious experiments on this acid, which they conſider as a peculiar kind. It has been admitted into the new chemiſtry under the name of the *formicine:* it is ſtronger than the vitriolic acid[*], ſerves for different combinations, and an ether may be obtained from it. M. Thouvenel, who prepared from it the formiate of potaſh, experienced the ſame effects from the volatile acid of ants as M. Roux; for, after having collected ſome of theſe inſects, large veſicles full of ſerous liquor were formed at the ends of his fingers. This learned chemiſt made many experiments to aſcertain the nature of the acid of ants, and they all convinced him that it differs from the mineral acids as well as from fermented vegetable acid, and that it has every chemical relation to the microcoſmic acid[†], &c.

M. Cadet junior, and other chemiſts, found that the volatile alkali deſtroys ants and neutraliſes their acid. This, therefore, may afford the means of being preſerved from it, or of checking its action on the ſkin. Olive oil produces the ſame effect.

Medicine might perhaps derive more advantage than it has hitherto done from the volatile ſpirit and oil of ants. A bath of ants has been preſcribed in caſes of the palſy. The water or ſpirit of magnanimity, and other pharmaceutic compoſitions, are prepared from ants and their products. They might certainly be employed in veſicatories.

To the above obſervations I ſhall add the following fact, recorded in the Ephemerides of the curious of Nature for the year 1688[‡], which will prove the efficacy of ants as a revulſive remedy:—A fuller, who had been long tormented with a pain in his head, and tired of taking medicines without receiving any benefit from them, reſolved to diſcontinue them, and to bear his ſufferings with patience. One day, however, while collecting ants, his hands were excoriated by the acrid liquor of theſe inſects, and he was immediately freed from the pain in his head, which never afterwards returned.

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[*] The author, no doubt, means that it will decompoſe ſome of the vitriolic ſalts: in this ſenſe many vegetable acids are ſtronger than the vitriolic.—EDIT.

[†] Phoſphoric acid.

[‡] Dec. 2, obj. 80.

XIV. *Experiments reſpecting the Influence which Oxygen has on the Germination of Seeds.* *By* Mr. SAUSSURE *junior*[*].

MOST philoſophers who have examined the nature of atmoſpheric air have found, that ſeeds expoſed to water and pure azotic gas do not germinate; that, on the other hand, carbonic acid gas is formed, which mixes with the azotic gas, and increaſes the atmoſphere of the plant. They obſerved alſo, that when oxygen gas was employed inſtead of azot, carbonic acid gas was in the like manner produced, but that the atmoſphere was then leſſened and the oxygen gas abſorbed.

Dr. Rollo[†], in the courſe of his reſearches reſpecting the germination of barley, remarked, that the formation of ſugar in the grains of barley during the proceſs of germination was the immediate conſequence of the action of oxygen gas on the grains, and that, in every caſe where this action was ſuppreſſed, no ſugar was formed. From this obſervation, as well as from the formation of carbonic acid gas in azotic and oxygen gas, he concludes, that oxygen gas is in part abſorbed by the barley, but that it in part forms with the carbon of the ſeeds carbonic acid gas. The ſugar which is formed immediately after the germination is, according to him, produced by the union of oxygen gas with the ſlimy vegetable fermentable matter.

If the quantity of the carbonic acid gas formed in this manner be leſs than that of the oxygen gas which has diſappeared, it is probable that a part of the latter is abſorbed by the grain, and that another part is employed in the formation of carbonic acid gas with the carbon of the grain. But if the quantity of the carbonic acid gas produced be greater than the quantity of oxygen gas which has diſappeared, we can only conclude, that the corn is capable of producing from its own ſubſtance, by the union of its carbon with the oxygen gas of the atmoſphere, a quantity of carbonic acid gas. If the quantity, however, of the oxygen gas which has diſappeared be perfectly equal to that which contributes to the formation of the carbonic acid gas during the progreſs of the germination, we may thence conclude that the oxygen gas is not abſorbed by the grain, but only employed in the formation of the carbonic acid gas. As no one has yet examined which of theſe three caſes actually takes place (it having been generally admitted that the oxygen gas is abſorbed by the grain during the progreſs of germination, without its being however proved), I made, towards the end of the year 1798, the following experiments at the temperature of between + 6 and + 12° of Reaumur in the ſhade.

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[*] From Scherer's *Allgemeines Journal der Chemie*, No. 7, for January 1800.

[†] It ſhould be Cruickſhank.

Experiment I.

I ſowed on a wet ſponge 21 peas, which together weighed 62 grains. The ſponge was introduced on a ſmall ſtool under a receiver which contained $13\frac{1}{2}$ cubic inches of atmoſpheric air purified by lime-water. The aperture of the receiver was cloſed by means of water, and the latter roſe in it to a ſufficient height to prevent the eſcape of the air during the change of the atmoſphere.

In eight days theſe peas had vegetated ſo much that roots were thrown out, of from three to four lines in length; the air in the receiver, in conſequence of the change occaſioned by the temperature and preſſure, was leſſened one-thirteenth part of its original volume. It now occupied 12·55 cubic inches, and lime-water indicated 0·10 of carbonic acid gas. The phoſphoric eudiometer gave 0·04 of oxygen gas, or 0·17 leſs than atmoſpheric air. A hundred parts of nitrous gas mixed in an equal quantity with the air in the receiver, left a reſiduum of 188 parts. The ſame mixture with atmoſpheric air, left a reſiduum of 105 parts. If we ſuppoſe, with

Lavoiſier, that atmoſpheric air contains $\frac{27}{100}$, or $\frac{6}{100}$ leſs oxygen gas than is ſhown by the phoſphoric eudiometer, the $13\frac{1}{2}$ cubic inches of atmoſpheric air, which were loſt in this experiment, contained before the peas were introduced 3·64 cubic inches of oxygen gas and 9·86 of azotic gas. The 12·55 cubic inches which remained after germination contained 1·255 cubic inches of carbonic acid gas, 1·255 of oxygen gas, and 9·86 of azotic gas.

If the decreaſe in the volume of the air during the germination ariſes from the carbonic acid gas being abſorbed by the water, which ſhuts the receiver and promotes the germination of the ſeeds, this decreaſe of volume with the oxygen gas which remains in the atmoſphere of the peas, ſhows the quantity of oxygen gas which has been formed in the courſe of the germination. If this quantity of carbonic acid gas is perfectly equal to that which is formed during the experiment by the union of the oxygen gas, which has diſappeared, with the carbon of the ſeeds, it proves, almoſt inconteſtably, that the oxygen of the atmoſphere is not abſorbed by the ſeeds, but has been employed in the formation of carbonic acid gas, a part of which is abſorbed by the water while the other remains in the atmoſphere of the ſeeds.

If we add the decreaſe of the volume of the air during the courſe of the germination, or 0·95 cubic inches to 1·255 cubic inches, the quantity of the carbonic acid gas which remains in the atmoſphere of the ſeeds, we ſhall have 2·205 cubic inches for the ſum of the carbonic acid gas. But this is almoſt the ſame quantity as that which is obtained by the union of the carbon of the ſeeds with the 2·385 cubic inches of oxygen gas which diſappeared in the courſe of the experiment.

It is very probable, therefore, from this experiment, that the decreaſe in the volume of the atmoſphere during the germination depends merely on the water abſorbing a part of the carbonic acid gas, which is formed by the union of the oxygen gas with the carbon of the ſeeds, and that it cannot be aſcribed to the abſorption of the oxygen gas by the ſeeds. To place the truth of this aſſertion in its full light, I examined whether the decreaſe of the atmoſphere of the ſeeds would take place if I endeavoured to prevent the carbonic acid gas from being abſorbed by the water.

If water be placed in an atmoſphere of pure carbonic acid gas, the water never abſorbs more of ſuch gas than what is equal to its volume, provided the fluid ſuſtains no other preſſure than that of the weight of the atmoſphere. The quantity abſorbed by the water is leſs, the greater the quantity of the atmoſpheric air mixed with the carbonic acid gas. When the carbonic acid gas makes only 0·02 of the atmoſpheric air, the portion abſorbed is not perceptible. This abſorption, therefore, may be always rendered imperceptible, either by increaſing the volume of atmoſpheric air which is in contact with the ſeeds, or by leaving in the receiver only as much water as is abſolutely neceſſary for the purpoſe of germination.

Experiment II.

Eighteen peas were placed in $11\frac{1}{2}$ cubic inches of atmoſpheric air, purified by lime-water, under a bell cloſed with mercury. The fluid roſe an inch higher than within the veſſel. Into this receiver I tranſmitted $\frac{1}{4}$ cubic inch of water in which the peas were to germinate: they floated half in it on the mercury. Ten days after, the peas had grown ſo much that their roots were from three to four lines in length. I examined the air under the bell, and found that its volume was not ſenſibly leſſened. Lime-water ſhowed 0.09 of carbonic acid, and the phoſphoric eudiometer, after deducting the carbonic acid, 0·12 of oxygen gas, and therefore 0·09 leſs than in atmoſpheric air. When nitrous gas was mixed in equal parts with this air, there remained a reſiduum of 132 parts. If we admit, with Lavoiſier, that atmoſpheric air contains about 0·27 oxygen gas, or 0.06 leſs than is indicated by the phoſphoric eudiometer, which in the air of my laboratory gives only 0·21 oxygen gas, it will be found that the air under the bell, before the peas were introduced, contained 3·105 cubic inches oxygen gas and 8·395 of azotic gas; and that after the germination the ſame volume of air contained 1·88 cubic inches of oxygen gas, 8·395 of azotic gas, and 1·035 of carbonic acid gas; conſequently, 1·225 cubic inches of atmoſpheric oxygen gas were employed to form the baſis of 1·035 cubic inch of carbonic acid. This reſult approaches as near the truth as can be expected in an experiment made with maſſes of air ſo exceedingly ſmall.

Experiment III.

I introduced into a receiver cloſed with quickſilver, 40 cubic inches of atmoſpheric air purified by lime-water. In this air I placed a conical glaſs with ſmall flints, which I moiſtened with two cubic inches of water, and depoſited on them four beans. At the end of fourteen days all the beans had thrown out roots of from four to five lines in length: I then took them out, and tried their atmoſphere. No ſenſible change in the volume was obſerved. Lime-water ſhowed 0·10 of carbonic acid gas: the phoſphoric eudiometer, after this gas was deducted, contained 0·13 oxygen gas, or 0·08 leſs than atmoſpheric air. This mixture with atmoſpheric air gave a reſiduum equal to 105 parts. According to this reſult, we find, if we add to the quantity of oxygen gas indicated by the phoſphoric eudiometer 0·06, that during the experiment 3·96 cubic inches of oxygen gas diſappeared, and were employed in the formation of four cubic inches of carbonic acid gas. The beans appear, therefore, in the atmoſpheric air to have abſorbed no oxygen gas.

Experiment IV.

I put 60 grains of barley, with $\frac{1}{3}$ cubic inch of water, into a receiver cloſed with quickſilver, and which contained 18 inches of atmoſpheric air purified by lime-water. The greater part of the grains of the barley remained adhering to the moiſtened ſides of the receiver: the reſt floated half under water upon the quickſilver. After the ſeeds had germinated, their atmoſphere was increaſed, the changes of temperature and the preſſure of a quantity imperceptible to the eye being taken into conſideration. Lime-water ſhowed $1\frac{1}{10}.\frac{5}{10}$ carbonic acid gas: the phoſphoric eudiometer, after deducting the carbonic acid gas, 0·11 of oxygen gas, or 0·10 leſs than atmoſpheric air. A hundred parts of nitrous gas, mixed in an equal portion with this air, left a reſiduum of 138 parts: the mixture with atmoſpheric air gave a reſiduum of 105 parts.

From this experiment it appears, according to the calculation more fully given above, that during the proceſs of germination 2·16 cubic inches of oxygen gas diſappeared and were employed for the baſis of 2·07 cubic inches of carbonic acid gas, which was found in the atmoſphere of the ſeeds. This reſult approaches near to that given by the compoſition of this gas.

But to obtain this reſult the ſeeds muſt all germinate, and they muſt be in immediate contact with the atmoſpheric air. When they do not germinate, either becauſe they are placed too cloſe to each other, are of a bad quality, or have too much water over them, the quantity of the carbonic acid gas formed will be greater than that produced by the union of the oxygen gas taken from the atmoſphere with the carbon of the ſeeds. Beſides, the experiment muſt be finiſhed before all the oxygen gas contained in the atmoſphere of the ſeeds has been converted into carbonic acid gas; for without this precaution the plants would be injured and decompoſed, and a greater quantity of carbonic acid gas would be found in their atmoſphere than would otherwiſe be the caſe.

The formation of ſugar in the ſeeds by the action of foreign oxygen gas is a very ſingular phenomenon which I will not venture to explain; I ſhall only remark, that from the above experiments the following concluſions may be drawn:

1. The oxygen of the atmoſphere is abſorbed during the proceſs of germination, not by the ſeeds, as hitherto ſeems to have been admitted, but is employed merely in forming carbonic acid gas with the carbon of the ſeeds.

2. The germinating ſeeds when in contact with the air do not form carbonic acid gas from their own ſubſtance, but only furniſh a component part of it, *viz.* carbon. They yield, however, from their own ſubſtance, the oxygen and carbon of the carbonic acid gas, formed when the ſeeds are in contact with water and pure azotic gas.

NEW PUBLICATIONS.

Geological Eſſays. *By* RICHARD KIRWAN, *Eſq.* F.S.S. *Lond. and Edin. Preſident of the Royal Iriſh Academy, &c. &c., and Inſpector-General of his Majeſty's Mines in the Kingdom of Ireland.* Bremner, Strand, 1799: 8vo. 502 Pages.

THIS work conſiſts of a Preface, an Introduction, twelve Eſſays, and a ſhort paper of Notes, following the laſt of the Eſſays. But, though its parts be thus arranged without ſyſtematic connexion in the formal ſtructure of the compoſition, it is, in fact, as to its ſubſtance, *one whole*; in which mineralogical ſcience is applied to explain all the appearances to which it can have any relation, on the ſurface or in the interior ſtrata of the earth. In the explanation of theſe appearances the author endeavours to aſcertain from its preſent ſtate what muſt have been the various former changes through which the matter of this globe has paſſed ſince the æra at which it was made ſubject to thoſe laws of exiſtence under which we now behold it. The following are the leading truths and opinions which occur in the ſucceſſive parts of the work.

The Preface mentions the diſcovery of the diſtinction between primary and ſecondary mountains, by Lehman, as one of the moſt important events in the progreſs of geological ſcience; expoſes the futility of the theory of Buffon, which repreſented all the primary mountains to have originated out of a ſtate of igneous fuſion; names with applauſe thoſe philoſophers who have ſubſtituted mineralogical and chemical inveſtigation for haſty conjecture in their attempts to trace the formation of the earth, and the changes which have happened in the arrangement of its ſtrata; profeſſes the intention of this work to be, to vindicate the divine authority of Revelation, by proving the account given by Moſes of the origin of the terreſtrial globe, to agree, in the moſt remarkable manner, with the faireſt inductions from the preſent phenomena.

The Introduction ſtates, that, in compoſing the following ſyſtem, the author has been careful "to attribute no effect to any cauſe of which the known powers are inadequate to its production," as well as "to alledge no cauſe whoſe exiſtence has not been proved to him by experience or credible teſtimony."

In the firſt Eſſay he obſerves that the earth could not have aſſumed the form of a ſpheroid compreſſed at the poles, if its ſuperficial parts had not originally been, to a certain depth, in a ſoft or liquid ſtate; and that the lower ſtrata of primitive calcareous rocks being, in proportion to their depth, continually thinner than thoſe which reſt upon them, equally evince the whole to have been formerly in a ſoft ſtate, and to have yielded to compreſſion while they were dried and hardened. He therefore ſuppoſes the ſuperficies of the globe to have been univerſally fluid, its interior compoſition ſolid and cavernous, at the time when the preſent laws began to operate upon it. The ſuperficial fluid he conceives to have been heated at leaſt to 33° Fahr. Diſſolved or ſuſpended in it were portions of the eight generic earths, of all the known metallic ſubſtances, of all the ſimple ſalts, and of all inflammable matters ſolid and liquid. Siliceous earth and iron were of all the ſolid materials in the mixture the moſt copious. Calcareous earth muſt even then have exiſted, for it is a component part in many primitive rocks. As the fluid maſs was moved; as its heat paſſed into new combinations; as the elective attractions of its various materials began reciprocally to operate; the ſolidifiable materials were precipitated in the order of their affinities, ſomewhat diſturbed by the mechanical agency, which at the ſame time neceſſarily affected them. In their precipitation they were univerſally cryſtalliſed. Where ſiliceous and argillaceous earths chiefly abounded, quartz, feld-ſpar, and mica, were confuſedly depoſited in ſtrata of granite and gneiſs. One depoſition ſucceeded another till vaſt uniform blocks were formed. Carbon, iron, and others of the ſolidifiable materials of the chaotic flood, were unavoidably involved with the ſubſiding ſtony particles. Where the materials were intermingled in different proportions, other ſorts of primitive rock, ſuch as ſiliceous ſchiſtus, ſiliceous porphyries, jaſpers, &c. were produced. In the confuſed agitation of all parts of the flood, ſulphur and metallic ſubſtances encountering, formed pyrites; while pit-coal was alſo compoſed by the aſſociation of petrol with carbon. After the formation of combuſtible compounds ſo eaſily decompoſable, heat ſufficient for their combuſtion was ſoon afforded from the inceſſant ſolidification of matters which had been hitherto fluid. A decompoſition of water, by means of heated iron, was one of the firſt effects from the riſing intenſity of the general heat. The combuſtible matters were generally inflamed, and many volcanic fires burſt out from the boſom of the abyſs. Gas-azot, gas-oxygen, and afterwards carbonic acid gas, were, in great quantities, evolved from the conflagration, and diffuſed into an atmoſphere ſurrounding the earth and waters. The eruptions of the fires naturally diſordered the depoſitions of the rocky ſtrata, and puſhed up many vertical or inclined elevations through the layers which might otherwiſe have been, in general, nearly horizontal. Such was the origin of the loftieſt primitive mountains, the Alps, the Andes, and thoſe elevated ranges which occupy ſo extenſive a ſpace in the north of Aſia. Plains were formed by the depoſition of ſtony particles from the chaotic flood in the intervals between the mountains, and with a diffuſion too looſe to admit of their being united in cryſtals as they were depoſited. The volcanic eruptions, by which the firſt mountains were elevated, could not but ſcoop out excavations to receive the ſubſiding waters of the ſea. The caverns of the primitive nucleus of the globe were even laid open by thoſe fires; and, into them, the ſuperficial waters ſhrunk, till a conſiderable extent of dry and ſolid ſurface was left bare under the atmoſphere. At a level of between 8,500 and 9000 feet above that at which it now ſtands, the ocean became for a while ſtationary. It was ſoon after furniſhed with fiſhes. The diſintegration of the primary rocks, the depoſition of animal exuviæ, and the continued precipitation of the ſolid matters ſuſpended in the waters of the ſea, then compoſed thoſe ſtrata of rocks which, from their containing animal remains, are denominated *ſecondary*. The earth was about the ſame time clothed with vegetation, and made the reſidence of the ſpecies of animals. The matter of the ſecondary rocks was, by the known laws of gravitation and reſiſtance, rather fixed in ſtrata upon the primary rocks than precipitated to the bottom of the great abyſs. The retreat of the ſea from the ſurface to the interior caverns of the globe, however, ſtill continued; and the dimenſions of the habitable earth were thus continually enlarged.—Such is the ſubſtance of the firſt of theſe Eſſays, which contains the author's theory of the formation of the earth. He compares this account with that of Moſes, and fancies that they perfectly coincide.

The Deluge is the ſubject of the ſecond Eſſay. From the ſhells found on the heights of the primitive mountains; from the remains of animals natives of hot climates diſcovered in cold ones, which thoſe animals cannot be ſuppoſed to have ever voluntarily inhabited; from the diſperſion of marine exuviæ on ſhores far remote from the ſeas where we now meet with the ſpecies of living animals to which they muſt have belonged; Mr. Kirwan infers, that the general deluge commemorated in the Holy Scriptures muſt certainly have taken place as Moſes relates. That the axis of the earth was originally parallel to the ecliptic, producing in every latitude perpetual ſpring, is a poſition of which aſtronomy has ſufficiently demonſtrated the falſehood. The nutation of the poles is, every nine years, retrogreſſive, and never exceeds ten degrees; ſo that the equator can never have been, by this cauſe, where the poles are now. The whole ſtrata of this earth are of ſuch nature that they cannot have originally been, as M. De Buffon ſuppoſes, in a ſtate of igneous fuſion, the heat from which might have long kept up a genial warm temperature even in the regions the neareſt to the poles. The phenomena ſufficiently evince that the ſeas and continents of the earth cannot have mutually changed places, as has been ſuppoſed by Mr. Edward King and M. De Luc. Mr. Kirwan, therefore, ſuppoſes the ſouthern ocean to have been in the general deluge driven, by ſome unknown means, with mighty force towards the poles. The character of the exotic remains found in the northern regions; the immenſity of waters collected in the ſouthern ocean; the traces which ſo many countries bear of having ſuffered ſome mighty ſhock from the ſouth; and the very figures of the great continents, ſharpened towards the ſouth, where they are waſhed by the ſouthern ocean, are ſo many grand facts concurring to give probability to that ſuppoſition. The waters from the central abyſſes of the globe might burſt forth to complete the cataſtrophe. In their progreſs northward, and in their retroceſſion, the waters of the deluge muſt have greatly broken, ravaged and tranſpoſed the ſuperficial ſtrata of the earth: many of the mountains were broken down to powder, and other inconſiderable fragments: the beds of the Euxine and Caſpian ſeas might be then ſcooped out: baſaltic rocks and beds of coal might be formed: a confuſion in the arrangement of the exterior ſtrata of ſecondary and primary rocks would be here and there produced: all terreſtrial animals, ſave ſuch as were preſerved by ſupernatural means, muſt have neceſſarily periſhed. Every rational inference from preſent appearances entirely coincides with the Moſaic hiſtory of the deluge.—This is the ſubſtance of the ſecond of theſe Eſſays.

In the third Eſſay it is ſtated, that, beſide effecting ſo many important and immediate changes on the exterior ſtrata of the globe, the deluge, likewiſe, became the remote cauſe of various great convulſions, which were at ſubſequent times to enſue upon its parts at different places of the ſurface: that the total ſeparation of Aſia from America, the coarctation of the Baltic, the opening of the Thracian Boſphorus, and the diſruption of the iſthmus which once divided the Atlantic Ocean from the Mediterranean Sea, the entire ſeparation of Great Britain from the continent, and various other ſimilar alterations are to be numbered among thoſe great changes poſterior to the deluge, for which the deluge had, however, prepared the ſuperficial ſtrata of the globe: that all theſe changes muſt have been accompliſhed chiefly by extraordinary agitations of the waters of the ocean, and at the diſtance of at leaſt three thouſand ſix hundred years from the preſent time.

The fourth Eſſay explains the phenomena of *lapidification.* Stones conſiſt of earths conglomerated. A lump of any earth of which the parts, though cohering, are diviſible merely by the nail of the finger, is called but *indurated earth:* if its coheſion firmly reſiſt the action of the nail, it is to be denominated a *ſtone*. Stones are hard which cannot be eaſily filed, or cut into pieces. Thoſe are firm which ſtrongly reſiſt percuſſion. Some ſtones appear to have been formed by cryſtalliſation arranging all their parts, internal and external, in a regular form. It is effected from either aqueous or igneous ſolution. All ſolidifiable matters are ſuſceptible of it. Even thoſe ſiliceous matters, which are now almoſt inſoluble in water, might be eaſily ſoluble in this menſtruum in their original ſtate of extreme diviſion, and were probably cryſtalliſed by depoſition from water. Nay, in ſpite of whatever Black and others may have affirmed to the contrary, numerous facts may be produced to ſhow that, even ſtill, water may be made to take up a certain proportion of this earth. Some ſtones appear to have been formed by the mere concretion of particles which in their primary compoſition are probably of different figures, and which therefore diſplay not in their coheſion the uniform order of cryſtalliſation. Other ſtones are formed by the cementation of exceedingly ſmall nodules, or even of larger ones, by the depoſition of ſtill particles amongſt them, and the coheſion of whole maſſes, partly by mechanical force, and in part by the reciprocal chemical attractions of the different parts. Many of the ſmaller nodules, which are found imbedded in rocks not of the ſame nature with themſelves, have been formed by an infiltration, in which water or ſome other fluid carried their particles through the ſuperficial ſtrata, but depoſited them after penetrating the interior layers only to a certain depth. Thoſe ſtones which we call petrefactions have been formed by the gradual ſubſtitution of ſtony particles into the places which were occupied by animal or vegetable matters that decay while the lapideous depoſition takes place. Theſe are all the different modes of lapidification. They are exemplified in all the different fragments, columns, and ſtrata of rock which the earth preſents to the obſervation of men.

In the fifth Eſſay, the *diſintegration,* reducing them into ſands and larger fragments is explained, as having been often produced by the abſorption of water, and by its congelation or rarefaction while it was diffuſed within them. The known external cauſes of the entire decompoſition of ſtony matters are repreſented to be only water, carbon, and carbonic acid gas, which enter into new combinations with the ſaline matters, the ſulphur, the ferruginous oxyds, the mangaueſe, the lime, and other conſtituent parts of the ſtones with which they come into contact.

Mountains are the ſubjects of the ſixth of theſe Eſſays. The author deſcribes the *primitive* mountains as containing within them no animal nor vegetable remains; as compoſing all the loftieſt extremities of the earth; as never covering nor leaning upon thoſe which are called *ſecondary*; as exiſting ſometimes in ſtrata, but oftener in huge rocks; as variouſly compoſed, of granite and granitic compounds, gneiſs, ſchiſtoſe mica, ſiliceous ſchiſtus, baſanite, jaſper, pitchſtone, hornblende, hornblende ſlate, indurated lithomarga, argillite, trap, ſerpentine and potſtone, porphyry, hornſlate, ſchiſtoſe porphyry, ſandſtone, rubble-ſtone, ſarcilite, granular limeſtone, marlite, gypſum, fluor, topaz-rock. Of each of theſe ſorts of ſtone he gives, in part, the mineralogical geography. The *ſecondary* mountains are next deſcribed by him, in the ſame eſſay, as being either marigenous or alluvial; as containing in their ſtructure various remains of animals and vegetables, terreſtrial and aquatic; as always incumbent on primary rocks, or inveſting them; as ſtratified or columnar, unigenous or polygenous; as variouſly compoſed of limeſtone, ſwineſtone, oviform limeſtone, porous limeſtone, marlites, and calcareous ſandſtones, chalk, gypſum, argillite, indurated clay, ſhale, bituminous ſhale, indurated lithomarga, ſlate, argillaceous ſandſtone, porphyry, trap, and baſalt; hornblende, argillaceous and calcareous breccias, hornſtone, jaſper, ſiliceous breccias and ſiliceous ſandſtone. By an ample induction of facts it is proved, that all theſe different ſorts of ſtone have been found in circumſtances in which they could not be but of ſecondary formation. This is the ſubſtance of that which ſeems to form, properly, the ſixth Eſſay, though the author has not ſo diſtinguiſhed it.

In the ſeventh Eſſay he deſcribes volcanic mountains as being generally of a conical form, of no exceedingly ancient origin, burning with no extraordinary intenſity of internal heat, and not half ſo numerous as ſome wildly theoretic mineralogiſts have ſuppoſed them to be.

In that which we ſhould incline to conſider as the eighth Eſſay, Mr. Kirwan endeavours to explain the internal arrangement of mountains. He here relates that their materials are diſpoſed either in irregular heaps or in beds or ſtrata, horizontal, varying from that poſition by an angle, of from 5° to 40°, or ſometimes even almoſt vertical. Sometimes the ſtrata run in the direction of the declivity of the mountains they compoſe, and ſometimes their courſe is directly oppoſite to this. Mountains of primitive limeſtone are often in irregular piles, but often, too, horizontally ſtratified. Of ſecondary calcareous ſtrata, the ſtructure is commonly horizontal. The ſtrata of ſecondary mountains, for the moſt part, aſcend towards the primary.—Theſe are ſome of the leading truths unfolded in this Eſſay.

Coal-mines are the ſubjects of the ninth Eſſay. Pit-coal is a compound of bitumen with carbon: its ſpecific gravity is from 1,23 to 1,500. The ſoils within which beds of coal uſually occur, are argillaceous, arenilitic, trappoſe, or calcareous. Theſe are diſpoſed in ſtrata over the coal, or in alternation with its ſtrata. The thickneſs of the ſtrata, or ſeams of coal, varies from half an inch even to 80 feet. Shale, bituminous ſhale, or ſandſtone, are the ſtones which generally form both the *roof* and *floor* to *ſeams* of pit-coal. Sometimes theſe ſeams are in their diſpoſition horizontal. They riſe at times to angles of from 25° to 45° with the horizon, It cannot be true that, as has been ſuppoſed by Genſanne, pit-coal is nothing but argillaceous matter impregnated with petrol or aſphalt; nor that this coal is entirely of vegetable origin; nor that it is formed from the fat and unctuoſity of the animals of the ocean. The ſtrata of this mineral ſeem rather to have been formed in conſequence of the diſintegration of many of the primitive mountains by the homogeneous union of the petrol and carbon which were by that diſintegration liberated out of the compoſition into which they had originally entered. Coal is therefore to be ſought, with probability of ſucceſs, only on the ſides of primitive mountains, in the vicinity of mountains of argillaceous porphyry, amidſt ſandſtone and ſhale bearing the impreſſions of vegetables, under alternating ſtrata of limeſtone and ſandſtone, or amidſt ſtrata of trap, or whin with ſandſtone or clay.

Common Salt and its Mines are the ſubjects of the tenth Eſſay. The ſea is here repreſented as owing its ſaltneſs to ſimple ſaline ſubſtances held in ſolution in it ever ſince the creation. The mines of rock-ſalt which have been in different places diſcovered, are ſaid to have all originated from the detention and evaporation of ſea-water. Salt-ſprings and lakes are related to contain and pour forth, from time to time, quantities of ſaline matter, which, when ſubjected to calculation, cannot but, by their magnitude, excite the greateſt aſtoniſhment. Theſe general facts are unfolded amid an exceedingly curious and intereſting diſplay of particulars.

The eleventh Eſſay is on the ſubject of Metallic Mines. It relates, that all the metals muſt have exiſted perfect and uncontaminated in the chaotic flood. Gold is found naturally pure oftener than any of the other metals. Silver is much more frequently mineraliſed by ſulphur, and alloyed with copper, antimony, lead, mercury, tin, or biſmuth. Nature preſents ſtill leſs of pure copper than of pure ſilver: the ores of copper are ſcarcely ever free from ſulphur. Of pure native iron there are few or no ſpecimens, except certain maſſes found in Siberia and Peru, which ſeem to have been originally agglutinated by petrol, and left bare by the diſſolution of the maſſes of other matters which once ſurrounded them. Native biſmuth accompanies the gray and the bright white ores of cobalt. Native arſenic is found only in the veins of primeval mountains: native mercury occurs in clay in mountains of ſecondary formation: ſulphurated ores have been formed at a time when ſulphur as well as metals were in a ſtate of extreme ſubdiviſion in which they were much more ſoluble in fluids than they are at preſent. The cryſtalliſation of metallic ores was the conſequence, in ſome inſtances, of the diminution of the watery menſtruum, in others, of ſlow precipitation. The veins of metals ſeem to have been, for the moſt part, formed by depoſition from water among the rifts and amid the ſtrata of the ſofter rocks. It is in oxyds, in mineraliſation by ſulphur, in mixture with different earths, and in amalgamation with one another, that all the metals are the moſt commonly found.—Such are the moſt general facts ſtated in this Eſſay.

The twelfth Eſſay is wholly controverſial. The late Dr. Hutton, of Edinburgh, had taught, that, from eternity, this globe had continued to paſs through a ſeries of revolutions, in which the land was ſtill deſtroyed by external influences, and, in its ruins, propelled into the ocean; while, on the other hand, internal fires were conſtantly elevating new mountains under the ocean, and conglomerating the materials driven into it; till, at laſt, the ocean changed its bed, that which had been dry land became ſea, that which had been ſea became dry land, and the deſtruction of the new continents, and the working of the volcanic fires under the new ſeas were again renewed. Mr. Kirwan ſoon attacked this theory as atheiſtical and unphiloſophical. Dr. Hutton warmly defended his own ſyſtem. In this concluding eſſay, as throughout the preceding parts of the preſent work, Mr. Kirwan, in reply to Dr. Hutton's defence, demonſtrates, that igneous fuſion cannot have produced thoſe phenomena which the Doctor aſcribes to it; affirms that volcanic fires are comparatively few and weak in their action; and maintains, that the compoſition of the ſuperficial ſtrata of our preſent continents is ſuch as could not poſſibly be formed at the bottom of the ſea.

The notes are few, and only mention ſome facts of which the author was, perhaps, not aware till it was too late to inſert them in the text of the Eſſays.

This analyſis has enumerated the leading general facts and poſitions of the preſent work. But theſe are founded upon a wonderfully extenſive induction of highly curious particular facts, for which the reader will do well to examine the book itſelf.

Theſe Eſſays certainly exhibit a more conſiſtent ſcheme of the facts and analogies of geology than any that had been before preſented to the world. They are written in a ſtyle eminently correct, manly, and elegant. If ſubſequent geologiſts ſhall continue to improve their ſcience as Mr. Kirwan has here done, the world may, perhaps, at ſome diſtant future time, ſee a faultleſs theory of the earth.

Phytologia; or an Eſſay on the Principles of Vegetation, &c. By ERASMUS DARWIN, *M. D. 4to.* Johnſon, Saint Paul's Church-Yard.

The Botanic Garden and Zoonomia, by Dr. Darwin, are two noble monuments of fancy, induſtry, and ſcience. In one we have almoſt the firſt inſtance of the happy application of poetry to illuſtrate and embelliſh the phyſical branches of philoſophy. The other is, to ſay the leaſt of it, a vaſt collection of the moſt curiouſly intereſting facts, phyſiological and pathognomical, in the hiſtory of human life, as well as of many of the moſt ingeniouſly conceived theories which have been propoſed to combine thoſe facts into ſyſtems, the foundations of practical rules.

In his Phytologia, the ſame author, with a flow of fancy and ſentiment nearly ſimilar to that in his former works, in the ſame ornate, yet clear, eaſy, and natural ſtyle, in the ſame characteriſtic method of aſſembling facts, deducing inferences, and ſuggeſting analogies, applies himſelf to explain the phyſiology of vegetables, to trace the chemical relations between vegetating bodies and the ſurrounding ſubſtances from which they are nouriſhed, and then to deduce a ſyſtem of rules for the improvement of the art of the gardener and the practice of the huſbandman.

The readers of the Botanic Garden may perhaps have ſuppoſed, that in the tale of the Loves of the Plants the author intended only to dignify and animate his account of the theory of Linnæus by a happy and beautiful poetic fiction. But they have miſtaken his purpoſe, and his botanical creed, who have thus conceived of them. He was never in his life more in earneſt, than in aſcribing to vegetables the ſenſations, the affections, the paſſions, and even the deſigns of which we, ordinary mortals, believe animals alone to be capable. In fact, it is one of his favourite opinions, that plants are truly animals. This opinion he ſtrenuouſly labours, throughout the phyſiological part of this volume, to illuſtrate and to prove. The ſeeds of plants he eaſily ſhows to reſemble the ova of animals. He diſcovers in the ſtructure of vegetables umbilical veſſels perfectly ſimilar to thoſe by which embryon and naſcent animals derive nouriſhment from the parent body, or from that ſtore with which the wiſdom of Nature involves them in the egg. He traces in the vegetable ſtructure, ſyſtems of abſorbents, glands, and blood-veſſels. He regards the leaves as lungs. He diſcovers the organs and the exerciſe, in theſe beings, of the moſt exquiſite ſenſibility. His facts are partly aſſembled from the works of almoſt all former writers on the phyſiology of vegetables, and, in part, the reſults of experiments ingeniouſly contrived and ſkilfully performed by himſelf. He ſuggeſts many pleaſing and ſpecious analogies, and here and there exhibits ſome clear and accurate deductions of new philoſophical truths. If we muſt, with whatever reluctance, own that he has not preſented to our minds ſufficient evidence of that full and perfect ſimilarity which he ſuppoſes between the ſtructure and functions of vegetables and thoſe of animals; it is, however, at the ſame time certain, that he has opened ſeveral fine veins of phyſiological reſearch, and that ſome of his novel poſitions, though not by himſelf ſatisfactorily proved, are ſuch that they may poſſibly receive complete proof hereafter.

In the ſecond part of his work he examines the chemical relations between the organiſed matter of vegetables, and the nutriment from which it is, by the vegetative functions, elaborated. He follows, with reſpect, the authorities of Prieſtley, Ingenhouz, Kirwan, and other eminent writers upon the application of chemiſtry to the ſubjects of gardening and agriculture. He truſts, however, as we ſhould judge, much too implicitly to the facts of Young, Marſhall, and other authors, whoſe publications on agricultural ſubjects, however popular, and however, in ſome reſpects, uſeful, contain few or no facts of ſuch preciſion and authenticity that a philoſopher can prudently take them for data without firſt verifying them by his own obſervations and experiments. Dr. Darwin is yet more ſuccefsful in explaining the chemical influences of ſoils, airs, water, temperature, and light, on the growth of plants, than in the eſtabliſhment of his new theory of their phyſiology. But in this chemical part of his work, he is content to repeat the principles and opinions of others without preſuming to propoſe any important new theory of his own.

The rules for the practice of the gardener and huſbandman, which he deduces as the reſults from both his chemical and his phyſiological reſearches, are partly diſperſed through the previous diviſions of the work, and partly aſſembled together towards the cloſe. They beſpeak ingenuity and good ſenſe, but do not ſufficiently wear the air of a practical acquaintance with farming, combined with accurate ſcience.

Throughout the work are ſcattered ſome curious diſquiſitions on ſubjects allied, indeed, to the primary deſign, but not entering directly and unavoidably into it. The whole will be read with great pleaſure by the admirers of Darwin's former writings.

Elements of Chemiſtry, &c. By ROBERT HERON. Longman and Rees, Paternoſter-Row. 8vo. Twelve Shillings.

This work explains, in nine books, 1. The general nature and hiſtory of chemical ſcience: 2. The nature, names, and peculiar uſes of the implements for chemical experiments: 3. The chemical properties and relations of thoſe ſimple and abundant chemical ſubſtances, caloric, oxygen, hydrogen, and azot: 4. The hiſtory of all the acidifiable baſes except the metallic, and of their acid compounds: 5. The hiſtory of the earths and alkalies, and of their neutral compounds with acids: 6. The nature and relations of metallic ſubſtances: 7. The hiſtory of bitumens and ſome other ſubſtances too complex and anomalous in their relations to be conveniently introduced into any of the foregoing branches of the arrangement: 8. The chemical hiſtory of the compoſition of animal and vegetable ſubſtances: and, 9. The application of chemical ſcience to the phenomena of geology, and to the functions of animal and vegetable life, &c. Several articles of appendix are employed in illuſtrating ſome things which had eſcaped the author's previous attention, in ſtating ſome theories which he does not yet preſume to rank as ſcience, and in conteſting ſome of the doctrines of Count Rumford and others.

This work has been favourably received by the public. Nor can we deny that the arrangement is convenient; that all the known principles and facts of chemical ſcience are in this book clearly ſtated; that the author has ſuggeſted many new things *which the diſcoveries of the ableſt foreign chemiſts are daily confirming*; nor, that this book will be found, perhaps, more eaſily intelligible by the mere novice in chemiſtry, than any other compendium of the elements of this ſcience with which we are acquainted.

The following are ſome of the leading new principles contained in it:

That lime is oxygen in a concrete form, and that the fixed alkalies are peculiar modifications of the ſame ſubſtance:

That, contrary to the aſſumptions of Count Rumford, caloric is a condenſible or elaſtic ſubſtance; and that, when fire is elicited by friction, there is an expanſion of condenſed caloric into a ſtate of extraordinary tenuity:

That, when it has been ſaid that water is a non-conductor of caloric, there has been an error, ariſing from the miſtake of a mechanical for a chemical effect, and from the uſe of indiſtinctly expreſſive terms:

That, in every function of organic bodies, the vital energy and the mechanical action of the organs are accompanied with certain chemical changes; and that the ſtudy of theſe chemical changes can alone guide us to the knowledge of a true theory and a rational practice of medicine:

That the electric fluid is, demonſtratively, a compound of caloric and light:

That, in a true ſyſtem of geology, the origin of the earth is to be traced from the homogeneous expanſion of all other matter in caloric, &c. &c.

INTEL-

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ROYAL IRISH ACADEMY, DUBLIN.

THE Royal Iriſh Academy have propoſed to give a premium of one hundred pounds ſterling to the author of the moſt detailed and accurate mineralogical and geological deſcription of the county of Dublin, with ſpecimens of the minerals found at an acceſſible depth.

The Academy alſo propoſes that a gold medal ſhall be given to the author of the beſt eſſay on the origin and progreſs of rhyme.

The Academy will alſo give a gold medal to the author of the beſt attempt to explain the Brehon laws of Ireland.

Eſſays on any of the ſubjects here propoſed will be received in either the Engliſh, French, Italian, or Latin language (if ſent poſt free) directed to the ſecretary, the Rev. Dr. Elrington, at their houſe, 107, Grafton-ſtreet, Dublin, any time previous to the 1ſt of November 1801: each eſſay to be inſcribed with ſome peculiar motto, and accompanied by a ſealed billet, ſuperſcribed with the ſame motto, in which ſhall be written the author's name and addreſs.

IMPERIAL ACADEMY OF THE SEARCHERS INTO NATURE AT ERLANGEN.

In the ſitting of the 23d of January laſt, the following new prize queſtion was propoſed:

To determine, in a ſatisfactory manner, by experiments, what mode of treatment, particularly in regard to temperature and the admiſſion of free air, is the beſt to be adopted in the ſmall-pox in general, and alſo in the inoculated ſmall-pox, and at the different periods of the diſeaſe? Whether diluting the matter with water before inoculation be attended with eſſential advantage; or whether too ſmall a number of puſtules be not by theſe means produced, and the body of the patient rendered liable to new infection? If it be true that rubbing olive-oil into the body be a certain preſervative againſt the plague, whether the ſame means might not be employed to prevent infection from the ſmall-pox? And what other kind of oil, when olive-oil is not to be had, might be ſubſtituted, with moſt advantage, in its ſtead?

The candidates are requeſted to tranſmit their papers, which the Society would prefer in Latin, but which may be written alſo in German or French, with any device at pleaſure, and accompanied with a ſealed note containing the name and reſidence of the author, to the preſident of the Academy at Erlangen before the 1ſt of September 1801. The prize, which is a gold medal weighing twenty ducats, will be adjudged to the author of the beſt paper on the above ſubject on the 5th of January 1802.

ROYAL ACADEMY OF SCIENCES AT BERLIN.

The following papers were read in the Academy in the end of the laſt, and beginning of the preſent, year:

September 19th, 1799, A ſecond diſſertation on the great advantage and different uſes of the maize plant, and on the bread prepared from it; by Profeſſor Bernoulli. 26th, On the origin of human knowledge, by Counſellor Selle.

October 3d, On a paſſage in the Meno of Plato, by Trembley. 10th, Experiments and obſervations made in the courſe of the year: 1ſt, on the uſe and cultivation of the white beet, and its abundance in ſaccharine particles; 2d, on the varieties of this root, which may be employed with moſt advantage in the preparation of ſugar. 17th, A journal of aſtronomical obſervations made at the obſervatory of Berlin ſince 1798, by Profeſſor Bode. 24th, Second diſſertation on the influence of ſigns in the formation of ideas, by Von Caſtillon. 31ſt, Grammatico-critical obſervations on Montaigne, by Baſtide.

November 7th, Obſervations on certain phenomena in regard to the motion of rivers and ſtreams, by Trembley. 14th, On the length of a pendulum to ſwing ſeconds at Berlin, by Profeſſor Burja. 21ſt, Third diſſertation on the impenetrability of light, by Profeſſor Engel. 28th, Second diſſertation on the art of clothing mankind, and on clothing conſidered under different points of view; by Von Goyon.

December 5th, On eatable muſhrooms, by Mayer. 12th, Obſervations on the attractive force of ſpheroids, by Trembley. 19th, On the principles of metaphyſics, by Ancillon.

January 9th, 1800. Second diſſertation on the prize queſtions of the claſs of the ſciences ſince the year 1772, by Baron Von Chambrier. 16th, Phyſical obſervations on the arts of deſign, by Von Burgſdorf. 23d, Obſervations on the calculations reſpecting the duration of marriage, and the number of married perſons; by Trembley. 30th, This being a public ſitting, Merian the director, as perpetual ſecretary, delivered an oration in French. The following papers, alſo, were read: Conſiderations on the principal definitions in the ſciences, by Ancillon: Hiſtorical panegyric on queen Sophia Charlotte of Pruſſia, by Erman: On yeſterday, to-day, and to-morrow, by Baſtide: Further obſervations on dreſs, by Von Goyon.

February 6th, On the proper meaning of the term *a great man*, by Teller. 13th, On the uſe of perukes among the ancients and in the middle ages to the preſent period, and on the origin of the word *peruke*; by Nicolai. 20th, Examination of the red water of a lake near Lubotin in South Pruſſia, by Profeſſor Klaproth. 27th, On different kinds of maize, by Profeſſor Bernoulli.

March 6th, On ſpace, by Baron Von Chambrier. 13th, On the origin of the names of nations, countries, rivers, cities, and families; by the Abbé Denina. 20th, On eatable muſhrooms, ſecond diſſertation. 27th, Profeſſor Bode preſented the fourth number of his Celeſtial Atlas.

April 3d, Diſſertation on free-will, by Von Boufflers. 24th, On the Circenſian games, by Mejerotto.

May 1ſt, Deſcription of a new Eaſt Indian plant called *Piſonia Georgiana*, by Profeſſor Wildenow. 8th, On ſome new mathematical diſcoveries, by Profeſſor Burja.

ELECTORAL ACADEMY OF THE USEFUL SCIENCES AT ERFURT.

In the ſitting of January 3d, Count Von Beuſt read an extract from a treatiſe, written by his ſon Frederick Auguſtus Leopold, Count Von Beuſt, of the Academy of Mines at Freyberg, on the amalgamation of ſilver ore as practiſed at the Halſbruck and Ultermuld works near that place. From this paper and the annexed tables, it appears that 79785⅜ cwt. of ore, which, according to the common mode of amalgamation, would have given only 31139 pounds 4¼ ounces of ſilver, produced, by the method employed at Freyberg, 38330 pounds 2⅝ ounces; conſequently, there was a ſaving of 1190 pounds 7⅜ ounces.

In this treatiſe the author obſerves, that the theory of amalgamation is much indebted to the important experiments of the Spaniard Don Ethovar, who proved, contrary to the opinion of ſeveral eminent chemiſts, that the muriatic acid, in a concentrated ſtate, diſſolves gold as well as ſilver when theſe metals are reduced to ſmall particles.

Profeſſor Trommſdorf read a continuation of his collections towards a chemical knowledge of mineral bodies. This part related chiefly to ſome precious ſtones from India which he had examined.

ROYAL ACADEMY OF INSCRIPTIONS, THE FINE ARTS, HISTORY, AND ANTIQUITIES, AT STOCKHOLM.

The Academy, in the ſitting of April 9, propoſed the following prize ſubjects for the year 1800.

Hiſtory.—A hiſtorico-critical diſſertation on the printed or unprinted works which have been written by individuals of the royal family of Sweden. The prize is a gold medal of the value of twenty-ſix ducats.

Foreign Languages.—What are the advantages or diſadvantages in regard to the general happineſs of mankind of the preſent as compared with the laſt century? The prize is a gold medal of the ſame value.

Antiquities.—Examination of the origin, nature, and object of the military and naval expeditions undertaken by the Swedes about the middle of the twelfth century. The prize is a gold medal of the value of fifteen ducats.

Inſcriptions and Devices.—1. A Latin inſcription for the Exchange at Stockholm. 2. Plan of a medal to commemorate the moſt remarkable events which have taken place in Sweden in the courſe of the preſent century. The prize is a gold medal of the value of twelve ducats.

The papers, &c. of the competitors muſt be tranſmitted to the Academy, in the uſual form, before the 20th of January 1801.

FRENCH BOARD OF LONGITUDE.

The following paper has lately been publiſhed, ſigned by Delambre the preſident, and Lalande the ſecretary, of the French Board of Longitude:

The tables of the moon are of equal importance to aſtronomy and to navigation: the moſt celebrated geometricians have exerted themſelves to illuſtrate the theory on which theſe tables are founded. The moſt aſſiduous labour of aſtronomy is, to obſerve with care all the movements of this planet, without which there would be no real geography, and which furniſh the navigator with the moſt infallible means of aſcertaining the poſition of his veſſel, of directing its courſe, and of arriving in ſafety at any determined point of the globe. In proportion as the Newtonian theory was thoroughly examined and underſtood, and as aſtronomical inſtruments and modes of obſervation were improved, the lunar tables were improved alſo. Mayer, uniting his own obſervations to thoſe of cotemporary geometricians, and ſelecting the moſt accurate obſervations, was enabled to compoſe tables, which having been ſince compared with nearly twelve hundred unpubliſhed obſervations, have been found wonderfully accurate. Maron, under the direction of Dr. Maſkelyn, made a new improvement in them by adding ſeveral equations which had been omitted but pointed out by Mayer, and by modifying the co-efficients of all the reſt.

But, notwithſtanding this care, theſe tables, ſo valuable towards the middle of the century, began to loſe progreſſively their correctneſs. Theory, to which recourſe was again had, ſhowed the cauſe of the error, and the method by which it might be rectified. The papers tranſmitted as anſwers to the queſtions propoſed by the National Inſtitute two years ago, and proclaimed in the public ſitting of the 15th of Germinal laſt, (April 5th,) have thrown great light on the equations lately invented for the motion of the apogeum and node. But aſtronomers at that time had not been invited to turn their attention to all the elements of which the lunar tables are compoſed: this labour would have been too much diſproportioned to the time propoſed for the competition. One ſucceſs often creates a deſire, and ſometimes furniſhes the means of obtaining another. What has been ſo happily executed already, proves the poſſibility of doing ſtill more, and of procuring to aſtronomy lunar tables more correct and more laſting. Nothing remains after eſtabliſhing the epochs, the ſecular motion, and their inequalities, but again to diſcuſs, by compariſon with a great number of accurate obſervations, the preciſe quantity of the different equations which enter into the calculation of the moon's place. This is the queſtion which the French Board of Longitude now propoſes to the aſtronomers of every country. The conditions to be fulfilled are:

1ſt, To diſcuſs and eſtabliſh, by compariſon with a great number of good obſervations, the value of the co-efficients of the inequalities of the moon, and to give for the longitude, latitude, and parallax of that planet, formulæ ſtill more correct and complete than thoſe which have ſerved as the foundation of the tables now in uſe.

2d, To conſtruct from theſe formulæ tables of ſufficient extent and accuracy for calculation.

The prize will be ſix thouſand francs (250*l.*) The Board of Longitude does not fix any period for the competition, and will adjudge the prize to the firſt perſon who ſhall have fulfilled the above conditions.

The competitors will addreſs their papers to the *Bureau des Longitudes, Palais National des Sciences et Arts à Paris.* They muſt not be inſcribed with the name of the author, but only with a ſentence or motto, and may be accompanied with a ſealed note, containing, beſides the motto, the name and addreſs of the author. This note will not be opened but in caſe the paper ſhall have been found entitled to the prize. The prize will be paid, without any formality, to the bearer of an order for that purpoſe, which will be delivered by the ſecretary.

FRENCH NATIONAL INSTITUTE.

The following is an account of the labours of the Claſs of the Mathematical and Phyſical Sciences during the third quarter of the year 8, as read by J. B. S. Delambre, ſecretary:

Among the memoirs of which we have to give an account, there are ſome, and theſe form the greater number, that cannot be appreciated but by thoſe who have particularly applied to the objects on which they treat. As they would require all the attention of the moſt experienced mathematicians, they are not very ſuſceptible of abridgement. Such, in general, are all the memoirs on pure analyſis and the higher parts of geometry. We muſt be excuſed, therefore, if we do not take ſo much notice of theſe works as their importance would require. We ſhall content ourſelves with announcing only, by their titles, a memoir of C. Niewport on the immediate integrability of differential equations to any number of variable ones; and two memoirs of Biot containing analytical reſearches, one on partial differential equations, and the other on vibrating ſurfaces.

We ſhall pay a little more attention to a memoir on mechanics, by C. Coulomb, which contains, beſides the analytical part, a ſeries of experiments, of which we can at leaſt give the reſults. The ſubject is, the reſiſtance of fluids when the motion is very ſlow. If a body impels on a fluid, or when it is impelled by the latter with a very ſmall velocity, ſuch, for example, as 0·3 yard, *per* ſecond, it will be found by experiment that the reſiſtance is in proportion to the ſquare of the velocity: but where the motion is exceedingly ſmall, for example, below 0·01 yard *per* ſecond, the reſiſtance is not merely proportioned to the ſquare, but to a complex function of the velocity. Suppoſing the velocity very ſmall, the quantity which repreſents the reſiſtance will alſo be very little. It becomes very difficult to eſtimate it by the uſual means, and ſtill more difficult to diſcover what belongs to the different terms of the formula. To accompliſh theſe points it would be neceſſary to have a kind of meaſure which might determine, with ſufficient accuracy, the ſmalleſt forces, and afford the means of varying the velocity at pleaſure, in order to render the different terms of the formula prevalent in turns.

All theſe advantages C. Coulomb has found united in the machine with which he meaſures the torſion of a wire ſuſpended in a vertical poſition. He has already derived great advantages from this inſtrument in other branches of phyſics, and he has given in former memoirs theorems reſpecting the force of torſion, or the means of eſtimating it in weight. With this aid he has demonſtrated the real law of reſiſtance where the motion is ſlow; a law which had eſcaped Newton, Bernoulli, and Graveſend. It reſults from his experiments:

1ſt, That where the motion is very ſlow, the reſiſtance is proportional merely to the velocity, and the term depending on the ſquare becomes inſenſible.

2d, That the height of the fluid above the body immerſed in it, does not ſenſibly increaſe the reſiſtance.

3d, That the reſiſtance ariſes only from the coherence of the parts of the fluid with each other, and not from their adheſion to the body immerſed in them.

4th, That the reſiſtance in limpid oil is ſeventeen times and a half greater than in water.

We ſhall omit ſeveral other reſults which will be read with pleaſure in the memoir itſelf, and only add, that the agreement between the different trials is ſo great as muſt aſtoniſh thoſe not acquainted with the accuracy and preciſion of the inſtrument with which C. Coulomb has enriched natural philoſophy.

C. Prony preſented to the Claſs the three firſt parts of a work entitled Mechanical Philoſophy, or, An Analyſis of the different Parts of Equilibrium and Motion.

Since the invention of the new calculus, the province of mechanics has been conſiderably enlarged. A great number of queſtions beyond the reach of the ancient geometry have been reſolved, and form at preſent ſo vaſt a body of ſcience that the mind, occupied with its developements and demonſtrations, can ſcarcely comprehend the whole. This inſpired C. Prony with the idea of compoſing a methodical table of all the reſults diſengaged from intermediary calculations. This is what he calls *mechanical philoſophy*. He compoſed this work from the leſſons and materials he collected for the Polytechnic School; and his object is to furniſh ſtudents with the means of placing together and arranging the different parts of inſtruction they have received. The author quotes the ſources from which he derived his information, but his work contains a great many things which belong to himſelf, either as to the matter, or manner in which it is preſented; ſuch, for example, as every thing that concerns equilibrium; a method of obtaining the fundamental equations of ſtatics without employing in any manner the theory of momenta; a general demonſtration which ſhows that the theorems reſpecting momenta are only a peculiar enunciation of the principle of virtual velocity; new formulæ for the equilibrium and preſſure of elaſtic fluids, taking into conſideration the variation of dilatibility, and formulæ which may give to the barometer more generality and more certainty in practice than has hitherto been the caſe in determining the height of mountains. The author alſo makes a very important application of his principles to the calculation of the force with which earth preſſes againſt walls. The formulæ of Prony are entirely new, and exceedingly ſimple.

In conſequence of two papers on the motion of the moon, read in the laſt public ſitting of the Inſtitute, and which were deemed worthy of a prize, Laplace has made a new and very happy application of the theories publiſhed in his *Mecanique Celeſte*. M. Burg, one of the two authors who gained the prize, had remarked a periodical equality in the moon's motion in latitude, and had found that the duration of this period was eighteen years. He, however, aſſigned no cauſe for it, nor gave any law. On the contrary, he requeſted both theſe from the author, who has explained in ſo complete a manner the ſecular inequalities of the moon. His expectation has not been deceived. Laplace, in conſequence of M. Burg's deſire, has examined the queſtion, and found that there exiſts in the lunar orbit a motion of nutation analogous to that of the terreſtrial equator. The extent of this nutation depends on the oblate form of the earth, and may throw new light on that important element, ſince the inequality thence reſulting varies from one to two, according as the flatneſs is ſuppoſed to be $\frac{1}{334}$ or $\frac{1}{230}$.

Another equation, depending on the ſame theory, affects the moon's longitude, and is of equal importance in enabling us to aſcertain the quantity of the earth's flatneſs. As long as the real cauſe was unknown, great uncertainty prevailed in regard to the uſe which ought to be made of this equation, and aſtronomers omitted it in their calculations. The exiſtence of it was proved by obſervations, but it was indicated by no theory. Laplace himſelf, having calculated by the principle of univerſal gravity, carried it only to 2″, that is to ſay, to a third of its real value. But ſince he has taken into account the nutation of the lunar orbit, he has found that it indicates the flatneſs to be nearly what it is known to be from other phenomena.

To aſcertain it ſtill better, he made C. Bouvard ſelect from his paper which gained the prize three months ago, thoſe obſervations beſt fitted for deciding the queſtion propoſed by M. Burg. This reſearch was attended with complete ſucceſs, and 440 deciſive obſervations have given 7″ 5 as the co-efficient of the new equation of latitude, which ſuppoſes a flatneſs of $\frac{1}{314}$. This is nearly what has been given by the variations in the length of the pendulum as obſerved in different latitudes. The meaſurement of degrees in France and Peru give ſtill ſomewhat leſs, and therefore it is demonſtrated that the flatneſs $\frac{1}{230}$, as found by Newton, ſuppoſing the earth homogeneous, is by far too much. The uncertainty which may yet remain reſpecting a quantity the cauſe of ſo much diſpute, will in future be confined within very narrow limits: three phenomena, of a very different kind, lead us to the ſame concluſion.

Thus the moon, which, by her eclipſes, made known to the ancient aſtronomers that the figure of the earth is ſenſibly round, ſhows us at preſent, by continued obſervation of her motion, the ſmall ellipticity of the terreſtrial meridian. But we muſt obſerve that this ellipticity, which is below $\frac{1}{300}$, is too little to be perceptible to our ſenſes even if we had an opportunity of ſeeing the whole ſhadow of the earth. We know, however, that we ſee only a very inconſiderable part of it disfigured by the penumbra and inequalities of the lunar diſk; ſo that it would be impoſſible for the moſt accurate eye to diſcover, in ſo ſmall an arc, the leaſt difference between the circular curve and that of an ellipſe ſo little flattened.

The obliquity of the ecliptic, which cauſes the different ſeaſons and the difference in the length of the days, is one of the moſt intereſting elements in theoretic and practical aſtronomy. Every year the aſtronomers examine it with care, by determining the ſolſtitial altitude of the ſun. Mechain and Delambre, the one at the national and the other at his private obſervatory, have employed in theſe nice obſervations the ſame inſtruments which they uſed in meaſuring the magnitude of the earth. Lalande the nephew, and Burckhardt, have made ſimilar and ſimultaneous obſervations at the College de France with inſtruments of the like kind, but a little larger. Theſe obſervations, whatever may be the cauſe, do not ſeem to be ſuſceptible of the ſame preciſion as thoſe which have given the meaſure of the meridian by means of the ſtars, but by taking the general reſult of more than 600 obſervations, made at Paris both this year and at the ſummer ſolſtice of the year 7, there will ſcarcely be a ſecond of uncertainty in regard to the real quantity of this eſſential element which enters into all our calculations.

Since we have had occaſion to mention the meaſurement of the meridian, we ſhall here add, that the aſtronomers by whom it was performed are now employed in printing their obſervations, that they may ſubmit, to the examination of the learned in all countries, the proofs of a labour which has already received the ſanction of the deputies of different nations aſſembled at Paris laſt year.

We have received a letter from Ciſcar and Pedrayes, the Spaniſh members of the commiſſion of weights and meaſures, by which it appears that his Spaniſh majeſty has received the ſtandards of the metre and kilogramme ſent to him by the Inſtitute; that he has given orders for their being preſerved with ſuch care as may enſure their authenticity; and that he has charged Lenoir and Fortin to make, with the ſame accuracy, four other ſtandards of the metre and kilogramme; and that this order has been ſucceſsfully executed. On this occaſion the king of Spain took a pleaſure in making himſelf acquainted with the new metric ſyſtem, and he declared that he ſhould be glad to ſee it adopted throughout all Europe. To render it more palatable to the Spaniſh nation, and to facilitate the comprehenſion of it to every one, an elementary memoir has been drawn up on the ſubject, of which we have received ſome copies. It contains a complete explanation of the new meaſures, with a Spaniſh nomenclature, better adapted to the genius of that language, which M. Ciſcar propoſes to ſubſtitute for the Græco-Latin nomenclature adopted by the French republic.

The following account of the labours of the Claſs of the Moral and Political Sciences during the laſt quarter, was read by C. Leveſque.

C. Anquetil continues to analyſe the manuſcript and printed memoirs preſented to the Academy of Belles Lettres by candidates for the prizes propoſed by that Society. He has read the analyſis of a memoir on the ſtate of the ſciences in France from the death of king Robert to that of Philip the Fair.

G. Gaudin has tranſmitted to the Claſs reſearches on the legiſlation of Solon and the government of Athens.

C. Mintelle, in a memoir on the beſt method of writing geographical names, has ſhown the advantage of adopting the names of places ſuch as they are uſed in the different countries to which they belong, and, when imperious neceſſity requires that they ſhould be disfigured in the French language, of writing them twice, firſt as they are pronounced in French, and then as they are pronounced by the natives of the country.

C. Buache has read a memoir on New Holland, which, he thinks, was known more than a century before the period of the diſcovery of it by the Dutch. He ſupports his conjecture by the account given by Peritſol, and an old chart of the Indian ocean preſerved in the Britiſh Muſeum. However defective this chart may appear, he is of opinion that it may furniſh information of great utility in regard to the diſcoveries that remain to be made in that part of the globe.

C. Veron Forbonnais has given an analyſis of the principles of the circulation of proviſions, and of the influence which ſpecie has on that circulation.

C. Bouchaud, in hiſtorical reſearches reſpecting the edicts of the Roman provincial magiſtrates, has neglected nothing that could be ſuggeſted to him by a ſubject which belongs to the ſcience of hiſtory as well as to that of legiſlation. Each province had its own government and laws, and the Roman republic, after having acquired an immenſe extent by its conqueſts, was never able to form the wiſe deſign of attaining to unity.

CONSTITUENT PRINCIPLES OF FIXED ALKALIES.

Guyton-Morveau, on the 26th of April, read, in the French National Inſtitute, a memoir on the conſtituent parts of fixed alkalies. He made his experiments with Deſormes, pupil of the Polytechnic School. Their concluſion was, 1ſt, That potaſh is compoſed of lime and hydrogen: 2d, That ſoda is compoſed of magneſia and hydrogen. From other experiments they conclude, 3d, That lime is compoſed of carbon, azot, and hydrogen: 4th, That magneſia is compoſed of lime and azot; and, conſequently, of carbon, hydrogen, and azot.

NEW METHOD OF DESTROYING INSECTS.

Crell has lately publiſhed, in his Chemical Annals, the following recipe for deſtroying earth-fleas, bugs, ants, &c. diſcovered by C. Catin:—Take black ſoap, of the beſt kind, one pound three quarters, the ſame quantity of flowers of ſulphur, muſhrooms two pounds, and ſixty meaſures of river or rain-water. Divide the water into two parts, one of which muſt be poured into a veſſel deſtined for that purpoſe: ſuffer the ſoap to diſſolve in it, and add the muſhrooms after they have been a little pounded. Boil the other half of the water in a kettle, and tie up the ſulphur in a bit of rag or piece of fine linen, and ſuſpend from it a ſufficient weight in order that it may ſink in the water. During the time the water is kept boiling, which muſt be at leaſt twenty minutes, ſtir it continually with a ſtick, and preſs the bag containing the ſulphur, that the latter may be forced out into the water and communicate to it the neceſſary ſtrength and colour.

When the liquor is taken from the fire, pour it directly into the caſk, and ſtir it round for a conſiderable time: the proceſs of ſtirring muſt be repeated daily till it acquire a fetid ſmell. Experience has ſhown that the more fetid the mixture is, its activity is the greater. Each time that the mixture is ſtirred, the caſk muſt be ſtopped immediately after. When you wiſh to uſe the liquid, nothing is neceſſary but to ſprinkle a little of it on the plants which you are deſirous of preſerving, or to dip their branches in it. It will be better, however, to make uſe of a ſyringe, having at the end a head, an inch or an inch and a half in diameter, pierced with ſmall holes. This inſtrument may be uſed for tender plants; when you apply the liquid to trees, a ſyringe with larger holes muſt be employed.

Caterpillars, beetles, earth-fleas, bugs, and the tree-lice which infeſt orange-trees, will be deſtroyed by the firſt application of the liquid. Inſects which reſide below the earth, ſuch as waſps, hornets, ants, &c. require that the liquid ſhould be ſquirted out gently, and without intermiſſion, that it may better penetrate to their neſts. Ants-neſts, according to their ſize, require from two to three meaſures of liquid, and in many caſes it muſt be applied for twenty-four hours. When the ants aſſemble in another place, the proceſs muſt be repeated. Two ounces of *nux vomica* may be added to the mixture and boiled along with the ſulphur. This ſubſtance, particularly when you wiſh to deſtroy ants, will be of great ſervice. When the whole of the liquid in the caſk has been uſed, the reſiduum muſt be buried in the earth to prevent domeſtic animals from eating it.

CEMENT FOR PRESERVING WOOD AND BRICK, &c.

The compoſition, or cement, is compoſed of the following materials; *viz.* tar, pulveriſed coal (charcoal is eſteemed the beſt), and fine, well ſlacked lime; the coal and lime to be well mixed together, proportioned at about four-fifths coal and one-fifth lime; the tar to be heated, and, while hot, thickened with the mixture of coal and lime, until it becomes ſo hard as that it may be eaſily ſpread upon the ſurface of a board, and not run off when hot. Turpentine or pitch will anſwer nearly as well as tar, and plaſter of Paris will anſwer inſtead of lime; to be uſed in the ſame manner, and in about the ſame proportions. The cement muſt be applied warm, and is found to be uſed eaſieſt with a trowel.

NEW REMEDIES FOR THE ITCH.

It has been obſerved that the workmen employed in the manganeſe mines at Macon, in France, are always free from the itch, and that thoſe even attacked by that diſeaſe in the neighbourhood, are accuſtomed to work in theſe mines in order to get rid of their malady. The itching, it is ſaid, in that caſe ſoon ceaſes, the eruption dries up, and the ſkin in a few days becomes perfectly clean.

C. Grille having remarked that the clothing of the labourers in theſe mines, and particularly articles of linen, became exceedingly white in the courſe of a little time, expoſed ſeveral pieces of coloured cotton cloth to the air diſengaged from the interior of theſe mines, and found, that in the courſe of a few days they had loſt a great deal of their colour. This led him to conjecture that manganeſe itſelf might be employed as a remedy for the itch. He therefore took ſix parts of pulveriſed manganeſe and ſixteen of hog's lard and formed them into an ointment, which he cauſed ſeveral perſons troubled with the itch to rub over their bodies; he adminiſtered alſo at the ſame time the uſual internal medicines, and the patients in a few days were perfectly cured.

Parmentier, who has made known this obſervation of C. Grille, adds, that in the courſe of his reſearches reſpecting the means of obviating the unpleaſant evaporation which proceeds from common ſewers, he obſerved that this evaporation preſerves thoſe who clean them from certain diſeaſes. Theſe people aſſert that they never know any thing of the itch; that they are ſo ſecure from it, that they can ſleep with itchy perſons without any danger of infection; and that itchy perſons who undertake their labour, get cured in the courſe of a few days. When theſe people ſcratch or cut their fleſh, the wounds heal up in twenty-four hours; it is remarked alſo that they are never troubled with chilblains or tetters, and that they have always a ſound ſmooth ſkin.

Theſe obſervations of C. Grille ſeem to confirm what has been ſaid in regard to employing vital air for extirpating the ſmall-pox.

KLAPROTH.

The eminent merits of Klaproth as a chemical analyſer, to repeat the expreſſion of Kirwan, of the moſt conſummate ſkill and accuracy, are acknowledged on all hands, and too well known to require encomium: it will therefore give our chemical readers great pleaſure to hear, that a *tranſlation of his Analytical Eſſays towards promoting the Chemical Knowledge of Foſſils* is now in the preſs. We underſtand it is the performance of the tranſlator of *Gren's Principles of Modern Chemiſtry ſyſtematically arranged*, of which we took notice in a former number.

DEATHS.

At Marburg, on the 12th of May, in the 47th year of his age, Leonard John Charles Juſti, public profeſſor of philoſophy and profeſſor of theology.

At Gottingen, on the 17th of the ſame month, in the 40th year of his age, of a fit of apoplexy, after ſuffering many years from hypochondriac affections, Chriſtopher Girtanner, M.D. He was a native of Swiſſerland, and was the firſt chemiſt in Germany who adopted the new chemical doctrine of the French. In 1791, he publiſhed at Berlin a work entitled *Neue Chemiſche Nomenklatur für die Deutſche ſprache*; The New Chemical Nomenclature in the German language; and the next year, The Elements of the Antiphlogiſtic Chemiſtry. He was the author alſo of ſeveral other works, and of a great many papers on chemiſtry and other ſubjects, printed in different journals.

THE

PHILOSOPHICAL MAGAZINE.

AUGUST 1800.

I. *Obſervations on the Straits of Malacca, in regard to Natural Hiſtory, Geography, and Commerce. By* C. HANSEL*.

THE iſland of Pulo Pinang, which lies cloſe to the coaſt of Queda, between the fifth and ſixth degree of northern latitude, and which now belongs to the Engliſh Eaſt India company, may ſerve as a proof how well the Engliſh underſtand the art of eſtabliſhing colonies, and bringing them in a ſhort time to a flouriſhing condition. About the year 1784, the Malay king of Queda made a preſent of the whole iſland to Mr. James Light, the captain of a country ſhip trading thither, and the latter ſold it to the Eaſt India company, with the proviſo that he ſhould be appointed governor of it; which was accordingly done, and he remained in that office till the end of the year 1795. The iſland was uninhabited, and entirely covered with wood. In order to erect a few houſes it was neceſſary, therefore, to deſtroy part of the wood, which was effected chiefly by means of fire.

This iſland, which was now called Prince of Wales Iſland, I viſited, for the firſt time, in the year 1793, conſequently nine years after it had been taken poſſeſſion of by the Engliſh. At that period a beautiful town, of a pretty conſiderable extent, and regularly laid out, had been built. A particular part of it was deſtined for the Malays and other Aſiatics, a great number of whom had already eſtabliſhed themſelves in the place. The market was well ſupplied with fruits, fiſh, and other neceſſaries. A conſiderable trade had alſo begun to be carried on. Ships from Bengal brought hither opium, rice, and cotton, which were bartered for pepper, tin, betel-nuts, and gold-duſt. All the company's ſhips touch here in their voyage to China, as well as the large fleets from Bombay and Madras. The arrival of ſo many veſſels makes this place a ſtaple, where buying, ſelling and bartering are continually going on. The harbour, which is formed by the coaſt of Queda and the eaſt coaſt of the iſland, is deep, and ſecure from every wind. Thoſe who are unacquainted with its geographical ſituation imagine they are entering a river; ſo ſhort is its diſtance from the continent. When viewed from the weſt ſide, the iſland appears to be round, and its mountains may be ſeen at a conſiderable diſtance. It is about thirty-five or thirty-ſix miles in circumference. The preſent inhabitants have applied chiefly to the cultivation of pepper, which is in a thriving condition, and promiſes to turn out very advantageous. As the iſland produces excellent timber for ſhip-building, docks have been formed, and a great many veſſels have already been launched.

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* From *Journal fur Fabrik, Manufactur, Handlung, und Mode*, October 1799.

A few miles from the town is a foreſt, which, riſing gradually, covers the mountains behind it. From the ſummit of the higheſt of theſe mountains a large ſtream of water projects itſelf more than a hundred feet, with a prodigious roaring noiſe, on the rocks below, where, after being collected in various natural baſons, it forms a ſmall rivulet, and then proceeds through the town, with a winding courſe, to the ſea. One can hardly conceive how pleaſant and refreſhing it is for thoſe relaxed by the heat of the climate to bathe in one of thoſe baſons, as the water is kept in a continual ſtate of coolneſs by the ſhady branches of the trees, which form over them a ſort of arches, and defend them from the rays of the ſun. The coolneſs of theſe groves, the majeſtic roar of the waterfall, and the noiſe of a variety of unknown inſects, make this an agreeable retreat to thoſe who have a taſte for the beauties of nature. In the year 1796, country-houſes were erected amidſt theſe fairy groves, which, in the courſe of time, may be converted into Engliſh gardens.

In the year 1789 the king of Queda took it in his head to demand back the iſland from the Engliſh; and, as the latter ſhowed no inclination to comply with his deſire, he aſſembled a conſiderable army on the coaſt oppoſite to the iſland, in order to lay ſiege to it in form. That he might inſpire his troops with more courage he gave them an entertainment, at which opium was not ſpared. His Malayan majeſty did not fail to get intoxicated along with his warriors; and this being reported to the Engliſh by means of their ſpies, they attacked the Malays in their camp, killed their king, made a great ſlaughter among them, and thus put an end to the war. A regular ſtone fort has ſince been built, in which a garriſon is conſtantly maintained of three battalions of ſepoys and Europeans.

At firſt, ſhips that touched here were obliged to ſail back the ſame way they had entered; which was attended with this inconvenience, that thoſe veſſels deſtined to proceed further through the Straits of Malacca could not take advantage of a fair wind, becauſe it blows directly into the harbour, and by theſe means five or ſix days were often loſt. In the year 1791, however, captain Popham * diſcovered a paſſage round the ſouth-eaſt end of the iſland, by which this inconvenience is obviated.

To ſail hence for Malacca there are two ways; one of which, for large ſhips, lies between two dangerous ſand-banks, where a continual and rapid current, which runs ither north-weſt or ſouth-eaſt, requires all the attention of

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* Captain Popham is the gentleman who commanded the expedition of the Engliſh to Oſtend. As far as I remember, he commanded, in the year 1791, a ſhip named *Il Truſco*, which had been fitted out at Oſtend by the Engliſh and ſailed under the Tuſcan flag. During the peace he obtained permiſſion from the Board of Admiralty, being then a lieutenant in the navy, to enter into the merchant ſervice; but as ſoon as the war broke out, he was again employed on board a ſhip of war.—H.

Captain Popham ſuperintended laſt year the embarkation of the Ruſſian troops deſtined for Holland, and aſſiſted the operations of the Britiſh army in that country. He received the honour of knighthood from the emperor Paul, being made a knight of Malta.—Edit.

the pilot to paſs in ſafety. Nature, however, ſeems as if deſirous to afford here every poſſible aſſiſtance, ſince the Aru iſlands on the weſt ſide, which conſiſt of ſome ſmall uninhabited rocks, ſerve the pilot as good marks to ſteer by. When theſe diſappear, the ſummit of the mountain Parcelar is diſcovered on the eaſt ſide, which muſt be kept in a certain direction, according to the current, in order to paſs without danger. As navigators between theſe ſand-banks have often to ſtruggle with bad weather and contrary winds, it is impoſſible to proceed but by continually heaving the lead, which is indeed attended with danger. The ſecond paſſage lies along the coaſt through the ſmall channel of Calum, which is ſcarcely ſo broad as the Elbe at Dreſden. Nothing more beautiful can be conceived than this paſſage, where veſſels ſometimes approach ſo near the coaſt that the bowſprit is often entangled among the branches of the trees. Birds of the moſt ſhining and variegated colours, unknown to the Europeans, are ſeen fluttering around, while others delight the ear with their ſong. As a ſand-bank in the middle ſhuts this paſſage againſt large ſhips, it is practicable only for thoſe which draw very little water.

When veſſels have reached the northern part, nothing can be more pleaſant than the navigation. They proceed along the coaſt, from which the wind, when it blows from the land, wafts along with it the moſt delicious aromatic odours. The mouths of the ſmall channels, the different windings of the coaſt, the bays which they form, and the land riſing like an amphitheatre, all together form the moſt enchanting proſpect. Nothing is wanting to complete the picture but habitations; for, in an extent of twenty or thirty miles, the only things of the kind that can be diſcovered, even with a ſpying-glaſs, are a few fiſhermen's huts.

At laſt, after weathering a headland, you ſuddenly diſcover the town of Malacca, formerly belonging to the Dutch. It lies at the head of a bay, and, though ſmall, is neat and well built. It is ſurrounded by beautiful gardens and country-ſeats. No place in the world produces more valuable fruits. The ananas here are half an ell in length and a quarter of an ell in diameter. They have an exceedingly ſweet melleous taſte. The mangoſtan, called in India the queen of fruits, is found here in great abundance, ſo that from two to three hundred of them may be purchaſed for a dollar. They grow only in the ſtraits of Malacca, Sunda, Banca, and in the iſland of Java. They are found neither at Madras nor Bengal, nor in the Philippines or China. The uſual fruits of India thrive here better than any where elſe. The bay abounds with well-taſted fiſh, and excellent oyſters, crabs, cray-fiſh, &c. Though this place lies nearly under the line, the climate is exceedingly healthful, and nothing is known here of thoſe fevers and diſeaſes to which other Aſiatic colonies are ſubject. The temperature of the air is cooled by the ſea and land breezes, which alternately prevail. The former begins between eleven and one in the forenoon, and blows very ſtrongly till towards ſix in the evening; at which time, however ſtrong it may be, the land-breeze aſſumes its place, and continues till about eleven the next day. By theſe means an agreeable coolneſs is always preſerved. At Calcutta, in Bengal, the heat is much more intenſe and inſupportable, though it lies at the diſtance of twenty-three degrees from the line; for it is not uncommon there to ſee Fahrenheit's thermometer, in the ſhade, during the months of April and May, at between eighty and ninety degrees. At the interval only when both winds ceaſe, the heat is ſtifling. The reaſon of the land-breeze here being ſo cool is, that, as Malacca is a peninſula, the wind always paſſes over a portion of ſea, whereas the land-breeze on the coaſt of Coromandel paſſes over the burning deſerts of Perſia.

The frequent and exceedingly violent thunder-ſtorms which take place here, contribute alſo very much to moderate the heat. They ariſe moſtly in the north-weſt, and bring with them ſuch coolneſs that I remember having been ſeveral times ſo cold that I was obliged to have recourſe to warmer clothing. The body here, indeed, is far more ſenſible of the ſmalleſt degree of cold than in the European climates, as the pores are kept ſo open by the heat. Theſe ſtorms are highly gratifying to the navigators bound to the Chineſe ſeas; for, as a ſouth wind, except in a very few places, where the ſea and land breezes ſucceed each other regularly, prevails in the ſtraits, they are glad to ſee ſuch ſtorms, which always begin to be formed in the north-weſt. In the night, however, they cannot be uſed with ſo much advantage, on account of the many ſand-banks, iſlands, and even the coaſt itſelf, which muſt be avoided. It has been remarked, that during theſe ſtorms the magnetic needle is in a continual ſtate of perturbation; and I have heard the captain of an Engliſh Eaſt Indiaman declare that he has ſeen, during a violent ſtorm, the north end of the needle point directly ſouth.

The European productions brought to Malacca for the purpoſes of trade are confined to a very few articles. Rice, opium, white and coloured cottons, form the principal part of them. The Malays bring hither pepper, nutmegs, ſago, rotangs, Spaniſh reeds, and gold-duſt. The latter article is firſt examined by the officers of the company, and then made up into ſmall packets in Chineſe paper, bound round with a thread and impreſſed with a ſeal containing Malay characters. Each packet contains a catty, and is worth from 460 to 500 piaſtres. The Spaniards, who go from the Philippines to Madras for the purpoſes of trade, touch here always in order to exchange their piaſtres for gold-duſt, which they carry with them to the coaſt of Coromandel, where, according to circumſtances, they gain two *per cent.* Theſe packets, when ſold, are never opened, but are taken on the ſeller's report; and no inſtance is known of any fraud ever having been practiſed on ſuch occaſions. The ſmall Spaniſh reeds are ſold for about eight piaſtres *per* hundred, and pepper at fifteen piaſtres *per* pickel of 17½ Spaniſh pounds. The cotton and opium brought hither are ſold moſtly to the Dutch, who ſell them afterwards to the Malays. The branch of trade, however, by which the inhabitants chiefly live, is ſupplying the ſhips which touch here with proviſions, freſh water, ſalted and freſh fiſh, poultry and fruit. The price of fowls is generally a piaſtre for eight or ten. Pine-apples are ſold at the rate of from four to ſix *per* piaſtre. Thoſe who are acquainted with the number of the ſhips which paſs through theſe ſtraits every year to China, the Philippines, the South Sea, Batavia, Borneo, Coringa, &c. may form ſome idea of the importance of this trade. The tavern where ſtrangers reſide is generally ſurrounded by a multitude of Malays having for ſale, apes, parrots, cowries, Spaniſh reeds, and various other articles. Among theſe people one may ſee many of their kings, who are diſtinguiſhed by a turban embroidered with gold, and who enter the tavern without receiving any particular marks of honour from the Europeans. The caſe is the ſame with all the petty Indian kings, of whom too high ideas are entertained in Europe, but who in India are looked upon as little better than corporals. It may be readily underſtood that I here ſpeak of the leſſer princes; for the greater ones, ſuch as the kings of Atcheen, Borneo, Pegu, &c. are in general treated with more reſpect.

The Dutch government, formerly eſtabliſhed here, conſidered it as of great importance to clear the ſtraits from the numerous Malay pirates by which it is infeſted. For that purpoſe it maintained a flotilla of cruiſers and flat-bottomed veſſels, which from time to time ſought out the Malays in their places of retreat behind the ſmall iſlands in ſhallow water, where their flat-bottomed veſſels were of excellent ſervice. The Malay prows generally carry a 24-pounder in the bow; have only one maſt, which can be lowered or raiſed at pleaſure; and are furniſhed with a rudder. They never attack a veſſel when there is wind, becauſe in that caſe it can manœuvre and make uſe of its cannon in every direction; but, if a calm take place, and they diſcover a ſhip from their retreats, they then come out and attack the veſſel behind and before, where they are out of reach of the cannon; maſſacre, with their poiſoned daggers called *kris*, all the white men on board; and carry off the blacks as ſlaves. A Dutch captain named Bloem, who commanded one of theſe cruiſers, was a terror to the Malays. Being acquainted with their moſt private retreats, he often ſurpriſed them when they leaſt expected it. They once imagined they ſhould be able to overcome him, and advanced againſt him, during a calm, with fourteen of their prows; but before they got near him a ſmall breeze ſprung up, ſo that he was enabled to direct his veſſel, and to give them ſuch a reception, that five or ſix of the prows were ſunk, and others had their rudders ſhot away; ſo that they were obliged, to avoid total deſtruction, to throw themſelves into the ſea, and to ſave themſelves by ſwimming. The reſt betook themſelves to flight; for the Dutch give the Malays no quarter.

Of the laſt inſtance of this man's courage, which coſt him his life, I myſelf was an eye-witneſs. The French frigate La Prudente, of thirty-ſix guns, on board which I was a priſoner, being in the ſtraits of Malacca, about two o'clock on the 17th of July 1795, the man at the maſt-head calling out *Vive la Republique,* announced that he ſaw a ſail which ſeemed to be ſteering towards us. As there was then little or no wind, the two veſſels approached each other very ſlowly, and it was therefore near ſunſet before we could perceive that the ſtrange ſail was a veſſel with two maſts. About ten at night, after I had lain down in my hammock, I was rouſed by the drum beating to quarters. The officers near whoſe birth my hammock was flung, immediately ſtarted from theirs, and deſired the *citoyen priſonnier,* meaning me, to do the ſame. The fore-deck was cleared, and every preparation made for battle. I then proceeded to the fore-deck, and obtained permiſſion to remain there. The two veſſels by this time were ſo near that they could ſpeak each other. Our commandant hailed the ſtrange ſail, and aſked to what nation ſhe belonged. The anſwer was, that ſhe was a Dutch ſhip of war.—Strike to the republican frigate La Prudente, and lower your ſails.—Are you really French? ſaid the Dutch captain.—Yes: take in your ſails and drop your anchor, or I'll ſink you.—I have no anchor on board, replied the Dutchman, but plenty of cannon; and at the moment we received a whole broadſide, which we inſtantly returned. Several of the Dutchmen being killed, they then called out that they ſtruck. Having taken poſſeſſion of the Dutch veſſel, and ſhifted the priſoners on board the frigate, we found the former to be the Java, of eighteen 9-pounders, commanded by the beforementioned captain Bloem, who had that day ſailed from Malacca. A ſhot had carried away part of his left breaſt, but he ordered that the colours ſhould not be ſtruck till after his death.

The inhabitants of Malacca are a motley race, conſiſting of Europeans, creoles, mulattoes, black Chriſtians (called commonly Portugueſe), Chineſe, and Malays. The Dutch and Germans eſtabliſhed here, all have mulatto or creole wives, for I do not know one whoſe wife is an European. The wife even of the laſt governor, Abraham Couperus, was a mulatto. The cuſtoms, manners, and dreſs of theſe ladies, are all in the Malay taſte. The dreſs which they uſe, both in the houſe and on public occaſions, conſiſts of a ſilk gown with wide ſleeves, which has a great reſemblance to a powdering-gown. Their hair is twiſted together and faſtened on the crown of the head with a ſilver pin. In the houſe their favourite female ſlaves ſit at their feet; and when they go out to walk, or pay viſits, they accompany them, bearing a ſmall ſilver box divided into different copartments, which contain certain articles indiſpenſably neceſſary for a Malay lady. Theſe articles are, betel, areka-nuts, chalk, a pair of ſmall tongs, and a knife for ſpreading the chalk on the betel-leaves, in which they wrap up ſmall bits of the areka-nut for the purpoſe of chewing them. The care which they beſtow on their domeſtic economy conſiſts only in ſeeing the orders which they give to their ſlaves carried into execution. When a ſtranger is invited to dinner, he is firſt introduced into an apartment where he is treated with a pipe of tobacco. Female ſlaves then make their appearance, one of whom brings him a ſilver baſon for waſhing the hands, another a veſſel containing water, and a third a towel. The company then ſit down to table, and are waited on by the female ſlaves belonging to the maſter of the houſe. They are attended alſo ſometimes by muſical ſlaves, who, during the entertainment, perform pieces of muſic.

Slaves here, both male and female, a few excepted, are treated with great ſeverity. The maſter of one of the taverns, a German, named Kreis, born in Hohenlohe, is particularly diſtinguiſhed in this reſpect. I never entered his houſe, which I did above ten times, without ſeeing puniſhment inflicted on ſome of his domeſtics. If the cook had not prepared the dinner according to his taſte, he was immediately carried into the back court, when two other ſlaves appeared with bamboos in their hands, and belaboured him on the poſteriors till his maſter, who walked up and down with his pipe in his mouth, told them to ſtop. If the ſlaves did not execute his orders with ſufficient ſeverity, he applied to their ſhoulders a bamboo, which he always on theſe occaſions carried himſelf. He always made it a rule when any ſtranger who happened to be in his houſe interceded for his ſlaves, to puniſh them doubly. Females ſlaves even were not excepted from this correction, which was inflicted in the moſt indecent manner. The Dutch government not only authoriſed this ſeverity, but even put it in practice itſelf. A poor ſlave belonging to the ſame Kreis had conceived an attachment to a female ſlave who reſided in the neighbouring houſe, and belonged to one Adrian Koek. The lovers could only viſit each other in the night-time, and for this purpoſe the former was obliged to clamber over the roof of a penthouſe. This nocturnal intercourſe being diſcovered, Koek complained to the fiſcal, and requeſted that the ſlave might be puniſhed. Kreis, therefore, was obliged to give up his ſlave, and the ſentence was, that he ſhould receive 500 blows with a bamboo, to be inflicted publicly. A ring was then put round each of his legs, between which an iron bar was fixed, ſo that when he walked he was obliged to deſcribe an arch with each foot, and in this ſtate was ſent back to his maſter.

On the other hand, I muſt obſerve that it is hardly poſſible, by the ſevereſt puniſhment, to reſtrain the profligacy and villainy of theſe men. What idea muſt we entertain of a man, who, having been ſeverely puniſhed for drunkenneſs, will, the very next day, take the key of the cellar from his maſter's cloſet and get ſo intoxicated as to be unable to ſtir from the ſpot? To a circumſtance of this kind I have myſelf been a witneſs. The puniſhment, indeed, was cruel; for the culprit was made faſt by the neck, middle, and legs, to iron rings faſtened in the earth, and belaboured till the blood guſhed from every part of his body. I was a witneſs alſo to a more ſtriking inſtance, which ſhows how little impreſſion good treatment makes on theſe uncivilifed people. An opulent Engliſhman, who had reſided ſome time at Malacca, taking a fancy to one of Kreis's ſlaves, purchaſed him for the ſum of 300 piaſtres. This ſlave was treated by his new maſter like a free man; he received money from him and permiſſion to go abroad on aſking leave, provided he remained to take care of the houſe in his maſter's abſence. But this ſlave, unaccuſtomed to freedom, notwithſtanding every threat and admonition, when he got out of the houſe, would not return in the courſe of the whole day, and at laſt, after being eight days in his new ſervice, he ſtole from his maſter a piece of money. His maſter endeavoured to make him confeſs the theft, with a promiſe of forgiveneſs for this firſt offence, but without ſucceſs. This circumſtance being told to Kreis, he conſidered it as a good opportunity of exculpating himſelf from the charge of cruelty brought againſt him by ſtrangers, as he imagined he could now prove that mild treatment to ſuch men would be entirely loſt. He offered alſo, not only to take back the ſlave, but to force him to confeſs. As the Engliſhman well knew that no one but his ſlave could be the thief, he delivered him over to Kreis, who tied him to a ladder, and then cauſed the uſual puniſhment to be inflicted. When he had received two hundred blows, his body was bent, and in that condition he was thrown into a hole till the next morning, when the puniſhment was renewed. He ſtill continued to hold out for ſome time, but Kreis having threatened to continue the puniſhment till he ſhould confeſs, he at length acknowledged the theft, but would not tell what he had done with the money. On the puniſhment being once more applied, he confeſſed that he had ſold the piece of money for a ſixteenth part of its worth to a Chineſe; who was immediately ſent for, and obliged to give it up. After this he remained in the ſervice of Kreis, who declared that he had never after any occaſion of complaint againſt him.

By what I have here ſaid, I do not mean to juſtify the cruelties exerciſed in this country, I only thought it neceſſary to ſhow, by ſome examples, how cautious people ought to be in forming opinions on this ſubject. The profligacy of theſe people is undoubtedly to be aſcribed to their maſters, who do not pay the leaſt attention to the formation of their moral character. They allow them to grow up like cattle, without taking care to give them any idea of religion, or of other knowledge, except what is immediately neceſſary to their maſters.

Thoſe who poſſeſs a whole family of ſlaves, never ſell any individual of that family, becauſe there have been numerous inſtances in ſuch caſes of the father's or mother's dying through grief, of becoming melancholy, or of deſtroying themſelves. The names given to theſe ſlaves are generally thoſe of the months in which they have been born or purchaſed. The commerce by ſea with the Malays is an armed commerce. All ſhips engaged in this trade carry a conſiderable number of guns, and from ten to twenty ſepoys or black ſoldiers. When a ſhip comes to anchor in any of the Malay ports, a beam is laid acroſs the deck before the mainmaſt to ſerve as a barrier between the buyers and the ſellers, and ſtrict watch is at the ſame time kept by the ſepoys, with their arms loaded and their bayonets fixed. If a Malay have any particular buſineſs to tranſact with the captain that requires longer time, he is admitted into the cabin in the after-part of the ſhip, but he muſt firſt ſuffer himſelf to be ſearched, to ſee whether he has about him a knife or any kind of weapons. Neglect of this precaution has occaſioned the loſs of many lives as well as veſſels. The treacherous Malay can never entertain any friendſhip for the Europeans; even if they ſhould live in habits of intimacy with him for a dozen or twenty years, when a favourable opportunity occurs of promoting his own intereſt, he will make no ceremony of doing it by ſacrificing his old friend. I could here give many inſtances of this kind, but I am convinced by what I have ſeen that the Europeans often give occaſion to this villainy by their own conduct. Being once on board a large Bombay ſhip, bound from Manilla to Madras, we diſcovered in the ſtraits of Malacca five large Malay veſſels, which, on diſcovering us, anchored between us and the land, except two, which were further out at ſea. As it was nearly calm, and the ſhip made little way, the captain, in a fit of intoxication, ſent one of his officers on board them to aſk whence they came, and whither they were bound. Their anſwer was, that they came from Rio, were bound to Malacca, and were laden with rotangs. This, however, did not ſatisfy the captain, and he immediately began to fire among them. Being much alarmed at this conduct, they cut their cables and rowed with all their might towards the land, where they were in danger of running on ſhore. In the mean time we paſſed the other two, which being without us were not able to eſcape, but, very fortunately, our captain did not obſerve them on account of his ideas being deranged, and becauſe his attention was directed to the other ſide. As ſoon as we had paſſed, all the Malays on board bent themſelves three times with their foreheads towards the deck, which in all probability was by way of returning thanks to Providence for the danger they had eſcaped.

In the ſtraits there are a great many iſlands which abound with excellent freſh water; but it cannot be at all times uſed, as the Malays poiſon the ſprings: to ſuch length do theſe men proceed in their hatred towards the Europeans. When ſhips enter the ſtraits, they are generally viſited by ſmall Malay boats, which bring fiſh and tortoiſes for ſale. The tortoiſes are of that kind called the green tortoiſe, which is three feet in length, about two in breadth, and weighs from two to three hundred pounds. A Spaniſh dollar, a ſmall quantity of rice, and a bottle of brandy, are in general the price of a tortoiſe and a certain number of fiſh. The fleſh of theſe tortoiſes is well taſted and much like that of veal, but difficult of digeſtion. The eggs, on the other hand, a hundred of which are ſometimes found in one animal, are a great delicacy, and made into ſoup. The greateſt advantage to ſeamen in purchaſing theſe tortoiſes is, that they live a long time; we kept many of them alive from three to four weeks on the deck without any nouriſhment, and without any other care than throwing ſea-water over them in the morning and evening. The whole ſtraits are a real paradiſe interſperſed with a multitude of ſmall iſlands. Thoſe who viſit them cannot help wiſhing that they could ſpend their whole lives on them, but while the Malays exiſt they muſt be uninhabitable for Europeans.

II. *Chemical*

II. *Chemical Experiments and Obſervations on the Preparation of Sugar from vegetable Productions found in Europe.* *By* SIGISMUND FREDERICK HERMBSTADT.

[Concluded from Page 113.]

9. *EXPERIMENTS with the* Beta vulgaris altiſſima, *the* Beta cicla altiſſima *of Jacquin, in order to procure Sugar from it.*

This beet, like the former, is very common, ſo that both kinds are uſed in Thuringia, and other parts of Germany, as food for the cattle. It is more juicy than the former, and has an exceedingly ſweet taſte. Thoſe which I employed for my experiments, grew at Schönberg, near Berlin, on an eſtate belonging to M. Nöldechen, member of the privy council. On cloſe examination, a great difference was obſerved in them; they were all covered on the outſide with a red rind, but in the inſide ſome of them were totally white; others conſiſted of red and white rings, and ſome of them of light red and dark red rings; many of them were very thick and tuberous, and others were almoſt as thin and long as the red beet. Whether this aroſe from their being of different kinds, or from the difference of the ſoil in which they grew, I cannot determine. Thoſe which I employed for my experiments, had grown in the ſame ſoil.

I took 112 of theſe beets, which weighed 125 pounds, and, having freed them from the tops and rind, grated them down, and when a portion of the juice had flowed off ſpontaneouſly, expoſed them to ſtrong preſſure. I obtained from them twenty-four quarts of very ſweet violet-coloured juice. In conſequence of what I had been taught by repeated experience, I firſt boiled it, without any addition, till a third part only remained, and at the ſame time a great quantity of albumen was ſeparated from it. When the liquor had cooled, it was ſtrained through a piece of woollen cloth, and being then mixed with twenty-four quarts of lime-water, was boiled for half an hour. At the end of that period it appeared as a clear liquor of a wine-yellow colour, and a great quantity of impure matter had been thrown up as ſcum. I again ſuffered it to cool, ſtrained it through flannel, and, being inſpiſſated, I obtained eight pounds of ſyrup.

A part of this ſyrup I poured into a deep evaporating diſh, into which I put ſome glaſs rods, and at the end of two months obtained, adhering to the rods, real ſugar, ſimilar to brown ſugar candy. As this ſyrup was much more pleaſant than common ſyrup, and exceedingly cheap, it might be uſed as a ſubſtitute for ſugar, like that procured from the white beet.

In continuing my experiments on the beet, I endeavoured to aſcertain the actual quantity of ſugar that might be procured from a determined quantity of them. With that view I weighed three pounds twelve ounces of ſyrup obtained by the ſecond experiment, poured it into a conical veſſel of tinned copper, and expoſed it to ſlow evaporation at the temperature of from 65° to 70° of Reaumur. In the courſe of eight hours there was formed on the ſurface a cryſtalline cruſt of granulated ſugar, which at the end of twenty-four hours was nearly four lines in thickneſs: having forced it down to the bottom of the ſyrup, in two days a new cruſt was formed, which was alſo forced down; and this operation I continued till a tough uncryſtalliſed pellicle only appeared on the remainder of the ſyrup: this induced me to conclude that all the cryſtalliſable ſugar was now ſeparated from the liquid, which was ſhown alſo by its being leſs ſweet and more ſlimy. I now put all the cryſtalliſed ſugar, together with the fluid ſyrup, into a ſmall ſugar mould, which I kept for eight days at the temperature of 30° of Reaumur, by which means the ſyrup was entirely drained off, and the ſugar remained almoſt in a dry ſtate. The whole of this operation employed thirty-ſix days. The raw ſugar I obtained was ſtill a little moiſt in the inſide, but it did not deliqueſce in the courſe of three weeks, during which I kept it in an open veſſel, and it weighed two pounds eight ounces: the ſyrup which drained from it weighed forty-eight ounces; conſequently, from the three pounds twelve ounces of ſyrup employed, twenty ounces of aqueous particles had evaporated. From this experiment it appears, that a buſhel of theſe beets, without any peculiar care being employed in the cultivation of them, will give 5^{1} pounds of brown raw ſugar, and $1\frac{7}{8}$ pound of not ill-taſted ſyrup, which, if not uſed as ſyrup, might be uſed with advantage for diſtilling ſpirits. I am now employed in refining my raw ſugar, and in determining how much fine ſugar and how much ſyrup will be produced from it.

10. *Experiments with the red Beet, the* Beta rubra, Beta cicla rubra *of Jacquin, to obtain Sugar from it.*

That it is poſſible to obtain ſugar alſo from the common red beets has been already proved by Margraf. I ſubjected them, therefore, to the like treatment as the preceding kinds, and obtained from a buſhel of red beets $6\frac{1}{4}$ pounds of ſyrup of a not unpleaſant taſte, but very inferior to that obtained from the white beet and the *beta vulgaris altiſſima.* I did not try to obtain ſugar from it. Margraf, from a pound of freſh red beets, obtained eight ounces of dry ſugar, and from thirty-two ounces of dry roots, about two and a half drams.

11. *Experiments with Carrots,* Daucus carotta *Linn.*

When carrots are cut in ſlices and boiled with water, you obtain a ſweet liquor, which by evaporation produces a ſweetiſh juice, which the country-people either ſpread upon bread or uſe to ſweeten different articles of food. Having freed a buſhel of carrots from the thin outer rind, I raſped them and expreſſed the juice. This liquor was then boiled, clarified with the white of an egg, and inſpiſſated to the conſiſtence of ſyrup. I obtained ſix pounds and a quarter of a not unpleaſant ſyrup, much inferior, however, to that obtained from the white beet and the *beta vulgaris altiſſima.* I found it impoſſible to ſeparate real ſugar from it: rectified ſpirit of wine extracted from it a ſubſtance which had a great ſimilarity to manna.

12. *Experiments with common Turnips,* Braſſica rapa, *to obtain Sugar from them.*

I took twelve turnips and raſped them, after having freed them from the tops and outer rind. The raſped matter had an agreeable, ſweet, and ſomewhat ſharp taſte, and, being properly expreſſed, gave a colourleſs ſweet juice. After clarifying it with the white of an egg, I ſtrained the clear liquor through a piece of woollen cloth, inſpiſſated it to the proper conſiſtence, and obtained an agreeable ſyrup of an inferior quality to that procured from the white beet, and *beta vulgaris altiſſima*; but equal to common ſyrup. By another experiment, I learned that a buſhel containing 125 roots, weighing 116 pounds, is capable of producing eight pounds of ſyrup. At the end of twelve weeks I found cryſtals of ſugar ſhot up in it; but they were exceedingly brown, and could be ſeparated only with difficulty from the remaining maſs.

13. *Experiments with the Cabbage Turnip,* Braſſica napobraſſica.

The juicy nature of theſe turnips, as well as the agreeable ſweet taſte of their juice, induced me to try whether I could obtain from them ſugar and ſyrup. Having peeled and raſped ſixty of them, which weighed 123 pounds, I obtained from them, by expreſſion, about twenty-two quarts of colourleſs juice, which was agreeably ſweet, but had at the ſame time a ſharp radiſh-like taſte. I boiled it twice; by which means a great deal of flaky matter ſeparated from it, and the liquor became perfectly clear. I filtered it after it had cooled, added to it twenty quarts of freſh lime-water, and boiled it ſlowly. While boiling, the ſharp radiſhy matter was evaporated, and I at length obtained ten pounds of tranſparent browniſh-yellow ſyrup, which had a perfect reſemblance to that obtained from the black birch, but which had the ſame foreign taſte. In order to procure ſugar from it, I put a portion of it into a veſſel in which I placed glaſs rods, ſuffered it to evaporate ſlowly, and at the end of ſome weeks found ſmall cryſtals of ſugar adhering to the rods, but the quantity I was unable to determine. As this turnip is dearer than the white beet and the *beta vulgaris altiſſima*, and gives a leſs pleaſant ſyrup even than common turnips, it muſt in this reſpect be conſidered as inferior to theſe three kinds.

14. *Experiments with the Skirret,* Sium Siſarum.

Margraf obtained from a pound of freſh ſkirret-roots eighteen ounces of dry ſugar; and half a pound of dry roots gave him three drams of real ſugar. The object of my experiments was to obtain merely a ſyrup fit for domeſtic purpoſes; but, on account of the many farinaceous particles which theſe roots contain, the preparation of dry ſugar from them, as Margraf remarks, was attended with many difficulties. As they cannot well be raſped, on account of their ſmallneſs, a buſhel of them, which weighed twenty-ſix pounds, was pounded in a ſtone mortar with the addition of a little water, and the juice expreſſed: the reſiduum was waſhed in cold water, and, being again expreſſed, I obtained a turbid ſweet liquor. I ſuffered it to remain eight days in a cool place, and found that a great part of the farinaceous matter had depoſited itſelf, and that the liquor was become clear and tranſparent. Being clarified with the white of an egg, and then inſpiſſated, I obtained five pounds of an agreeable ſyrup of a bright brown colour. From this it appears that theſe roots are too dear to obtain from them a cheap ſyrup.

15. *Experiments with Parſnips,* Paſtinaca ſativa Linn.

The peculiar ſweetneſs of this woody root induced me to ſubject it to a ſimilar examination, though Margraf had before obſerved that little ſugar was to be obtained from it. With this view I treated, in the ſame manner as the preceding, a buſhel of the roots, which weighed twenty-four pounds, and obtained 5½ pounds of an agreeably taſted ſyrup, but which had not entirely loſt its natural taſte. Had my other occupations admitted, I ſhould have endeavoured to aſcertain the proportional quantities of ſugar contained in determinate quantities of theſe ſubſtances; but as this has been impoſſible, I muſt reſerve them for another opportunity.

III. *Account of a Series of Experiments, undertaken with the View of decompoſing the Muriatic Acid.* By Mr. WILLIAM HENRY*.

MODERN chemiſtry, notwithſtanding its rapid advancement during the few laſt years, ſtill preſents to its cultivators ſeveral intereſting objects, both of analytic and ſynthetic inquiry. Among the former, the decompoſition of the muriatic and of certain other acids holds a diſtinguiſhed place; for our curioſity reſpecting the nature of theſe bodies is ſtrongly excited by the influence which the diſcovery would have on the general doctrines of chemical ſcience, as well as on the explanation of individual facts. The theory of the formation of acids, for example, one of the moſt important parts of the new ſyſtem of chemiſtry, muſt be regarded as incomplete, and liable to ſubverſion, till the individual acids now alluded to have been reſolved into their conſtituent principles. To the beſt of my knowledge, however, we are not in poſſeſſion of a ſingle fact that gives the ſmalleſt inſight into the conſtitution of the muriatic acid; and the attempts to effect its analyſis can only therefore be directed by the analogy of the decompoſition of other bodies, which, from ſimilarity of character, are arranged in the ſame claſs.

One of the firſt objects in the analyſis of a compound body ſhould be, its complete ſeparation from all other ſubſtances, which, by their preſence, may tend to introduce uncertainty into the reſults of the proceſſes that are employed. But it is ſeldom that a ſimplicity ſo deſirable can be obtained in the objects of chemical reſearch; for, agreeably to a known law of affinity, the laſt portions of any ſubſtance are ſeparated with peculiar difficulty; the force of attraction appearing to increaſe as we recede from the point of ſaturation. In a liquid ſtate, the muriatic acid is a totally unfit ſubject for analytic experiment; for, in the ſtrongeſt form under which it can be procured, it ſtill contains a large proportion of water. This watery portion, beſides the complexity which it introduces into the reſults of experiments, prevents any combuſtible ſubſtance that may be applied, from acting on the truly acid part; becauſe that claſs of bodies, having leſs difficulty in attracting oxygen from water than from the acid, will neceſſarily take it from the former ſource. The ſtate of gas, therefore, is the only one in which the muriatic acid can become a proper object of analyſis.

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* From *Tranſactions of the Royal Society of London for* 1800.

In the ſeries of experiments on this gas, which I am now about to deſcribe, I employed the electric fluid, as an agent much preferable to artificial heat. This mode of operating enables us to confine accurately the gaſes ſubmitted to experiment; the phænomena that occur during the proceſs may be diſtinctly obſerved; and the compariſon of the products, with the original gaſes, may be inſtituted with great exactneſs. The action of the electric fluid itſelf, as a decomponent, is extremely powerful; for it is capable of ſeparating from each other, the conſtituent parts of water, of the nitric and ſulphuric acids, of the volatile alkali, of nitrous gas, and of ſeveral other bodies whoſe components are ſtrongly united. I began, therefore, with examining attentively the effects of the electric muriatic acid gas without admixture *.

SECTION I.

On the Effects of Electricity on Muriatic Acid Gas.

When ſtrong electrical ſhocks were paſſed through a portion of muriatic acid gas, confined in a glaſs tube over mercury, the following appearances took place. The bulk of the gas, after 20 or 30 ſhocks, was conſiderably diminiſhed; and a white depoſit appeared on the inner ſurface of the tube, which conſiderably obſcured its tranſparency. In ſome inſtances, both the contraction and depoſit were much more remarkable than in others. The gas which iſſued from muriat of ſoda, ſoon after the affuſion of ſulphuric acid, and while the charge was yet warm, exhibited theſe appearances in an eminent degree. Of this gas, 307 meaſures were reduced, by 20 ſhocks, to 227, or were contracted nearly one-fourth. Gas from the ſame materials, after they had continued working for ſome hours, was diminiſhed, by ſimilar treatment, only about a twelfth. Theſe effects, therefore, it ſeemed probable, depended in ſome meaſure on the preſence of moiſture; and I accordingly found, that muriatic acid gas, after more than a week's expoſure to muriat of lime, brought into contact with it immediately after cooling from a ſtate of fuſion, was ſcarcely diminiſhed at all; and that the depoſit, though it ſtill occurred, was leſs copious in quantity. This depoſit was not, like corroſive ſublimat, ſoluble in water; but had every property of the leſs ſaturated ſalt, calomel.

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* The gaſes ſubmitted to the action of electricity, in the following experiments, were confined in ſtraight glaſs tubes of various diameters, armed at the ſealed end with a conductor of gold or of platina, but generally of the latter metal. The ſhocks were as ſtrong as could be given without breaking the tubes, which, notwithſtanding every precaution, were often ſhattered by the force of the exploſion. Each meaſure of gas is equal to the bulk occupied by a grain of mercury.

The mercury by which the muriatic acid was confined was therefore evidently oxydated; and to the combination of a part of the gas with the oxyd thus produced, the diminution of bulk was doubtleſs to be aſcribed. But it was uncertain from whence this oxygen was derived. It might either reſult from the decompoſition of the acid gas, or of the water chemically combined with it. The following experiments were therefore made to determine this point.

Experiment 1. Through 1457 meaſures of muriatic acid gas 300 electrical ſhocks were paſſed. There remained, after the admiſſion of water, 100 meaſures of permanent gas, (or not quite ſeven from each hundred of the original gas,) which, on trial, appeared to be purely hydrogenous.

Exper. 2. Of the gas dried by muriat of lime, 176 meaſures received 120 ſhocks. The reſidue of hydrogenous gas amounted to 11 meaſures, or rather more than 6 *per cent.*

Theſe experiments, and other ſimilar ones, made on comparative portions of muriatic acid gas in its recent ſtate, and after expoſure to muriat of lime, convinced me that it was impoſſible, by this method, wholly to deprive the muriatic gas of water. The recent gas, however, when electrified in ſmaller quantity than in experiment 1, gave a larger proportion of hydrogenous gas; which ſhows, that ſome portion of its moiſture was removed by expoſure to muriat of lime. In order, if poſſible, to procure the gas perfectly dry, another mode of preparing it was reſorted to. Alum and common ſalt were firſt well calcined, ſeparately, to expel their water of cryſtalliſation, and, being then mixed, were diſtilled together in an earthen retort. The gas proceeding from theſe materials was received over dry mercury; but, though only the laſt portion that came over was reſerved for experiment, it ſtill, after the uſual electrifation, afforded a product of hydrogenous gas.

In the courſe of the preceding experiments, I obſerved that the diminution of the muriatic acid gas ſtopped always at a certain point, beyond which it could not be carried by continuing the ſhocks. Gas alſo, which had been thus treated, when transferred to another tube, and again electrified, did not exhibit any further depoſit. It became intereſting, therefore, to know whether the production of hydrogenous gas had a ſimilar limitation; becauſe the deciſion of this queſtion would go far towards aſcertaining its ſource. If the evolved hydrogenous gas aroſe from the decompoſition of the acid, it might be expected to be produced as long as any acid remained undecompoſed. But if water were the origin of this gas, it would ceaſe to be evolved, when the whole of the water contained in the gas had been reſolved into its conſtituent principles.

Experiments 3 and 4. Into two ſeparate tubes I paſſed known quantities of muriatic acid gas. Through the one portion 200 diſcharges were taken, and through the other 400. On comparing the quantities of hydrogenous gas produced, it proved to bear exactly the ſame proportion, in each tube, to the gas originally ſubmitted to experiment. Hence it may be inferred, that the hydrogenous gas, evolved by electrifying the muriatic acid, has its origin, not from the acid, but from the water which is intimately attached to it. The agency of the electric fluid appears alſo, from the following experiments, to be exerted, not only in diſuniting the elements of water, but in promoting the union of the evolved oxygen with muriatic acid.

Exper. 5. A mixture of common air and muriatic acid

gas,

gas, in the proportion of 143 of the former to 116 of the latter, was rapidly diminiſhed by electrical ſhocks; 30 of which reduced the whole to 111 *. The remainder conſiſted of muriatic acid and azot gaſes, with a ſmall proportion of oxygenous gas. The depoſit formed on the tube was of the ſame kind as before, but much more abundant.

Exper. 6. The ſame appearances were occaſioned, much more remarkably, by electrifying muriatic acid with oxygenous gas; and the contraction continued till the mercury roſe ſo as to touch the extremity of the platina conductor. At each exploſion, a denſe white cloud was ſeen in the tube, which ſoon ſettled on its inner ſurface, and was of exactly the ſame chemical compoſition as the one already deſcribed. Nitrous gas and muriatic gas, when electrified together, underwent a ſimilar change.

In order to aſcertain whether the mercury by which the gaſes were confined, in the above experiments, had any influence on their reſults, they were repeated in an inſtrument made, purpoſely for the occaſion, by Mr. Cuthbertſon, of London. It conſiſted of a glaſs tube, ground at each end, with the view of receiving two ſtoppers, each perforated with platina wire, which projected into the cavity of the tube. When the ſtoppers were in their places, the extremities of the wires were at the diſtance of about half an inch; and, by properly diſpoſing the apparatus, electrical ſhocks might be paſſed, through any gas or mixture of gaſes, with the contact only of glaſs and platina.

Exper. 7. In this tube I electrified the muriatic acid gas, and then admitted to it an infuſion of litmus. The ſudden deſtruction of its colour evinced the formation of oxygenated muriatic acid. Not the ſmalleſt depoſit appeared on the tube.

Experiments 8 and 9. The ſame phænomenon took place when an infuſion of litmus was brought into contact with a mixture of common air and muriatic acid, and of oxygenous gas and muriatic acid, after electriſation in this inſtrument; oxygenated muriatic acid being produced in both caſes.

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* This experiment ſuggeſts an additional reaſon to that already given for the greater diminution of the firſt than of the ſubſequent portions of the muriatic acid gas; for the former may be preſumed to have been much more adulterated, than the latter, with the atmoſpherical air of the veſſels.

The above facts prove that the combination of oxygen with muriatic acid, in theſe experiments, is not occaſioned by a pre-diſpoſing affinity in the mercury to combine with oxygenated muriatic acid; but that the electric fluid ſerves actually as an intermedium in combining the muriatic acid with oxygen.

From the relation of theſe experiments it appears, that not the ſmalleſt progreſs had been made by them towards the decompoſition of the muriatic acid. I reſolved, therefore, to attempt its analyſis, in a ſimilar manner, with the aid of combuſtible gaſes.

SECTION II.

Effects of electrifying the Muriatic Acid Gas with inflammable Subſtances.

In a memoir read before the Royal Society, and inſerted in their Tranſactions for 1797, I have ſhown that, when electrical ſhocks are paſſed repeatedly through a confined portion of carbonated hydrogenous gas, the water held in ſolution by the gas is decompoſed by the carbon, which forms a conſtituent part of it; that carbonic acid is formed; and an addition made, of hydrogenous gas. Hence the bulk of the carbonated hydrogen gas is conſiderably enlarged by this proceſs, which ſhows, by its reſults, that the affinity of carbon for oxygen is rendered much more powerful and efficient by the electric fluid. I have ſince found that other oxygenated ſubſtances are decompoſed by electrifying them with carbonated hydrogen gas. Nitrous gas, for example, is ſpeedily deſtroyed by this proceſs, and carbonic acid and azotic gaſes are obtained.

Every attempt to decompoſe the muriatic acid muſt be founded on the preſumption that it is an oxygenated ſubſtance; and thoſe bodies promiſe to be the moſt ſucceſsful agents, that poſſeſs a ſtrong affinity for oxygen. Now, of all known bodies, charcoal moſt ſtrongly attracts oxygen; and I have, therefore, repeatedly attempted the deſtruction of this acid, by paſſing it over red-hot charcoal. But, in a ſeries of experiments, which I made ſome time ſince, with this view, in conjunction with Mr. Rupp, we ſoon found reaſon to be diſſatisfied with the difficulty and uncertainty of this proceſs. An immenſe production of hydrogenous gas took place; but it was not eaſy to determine whether it had its origin from real acid or from water. Our experiments, however, though inſufficient to furniſh deciſive proof, induced us to believe that it had the latter origin.

It next occurred to me, that the comparative affinities of the muriatic radical, whatever it may be, and of charcoal, for oxygen, would be elegantly and ſatisfactorily aſcertained, by electrifying together the carbonated hydrogen and muriatic gaſes. If the muriatic acid be capable of decompoſition by carbon, it might be expected to be deſtroyed by this proceſs; and the exact quantity of acid decompoſed, and the nature and quantity of the products, would thus be eaſily determined. I electrified, therefore, the muriatic acid and carbonated hydrogen gaſes, with the moſt ſcrupulous attention to the phænomena and reſults. That the electric fluid might not be miſapplied in decompoſing the water of the carbonated hydrogen gas, it was kept more than a week, before uſe, over quicklime, introduced to it while yet hot.

Exper. 10. Of this carbonated hydrogenous gas, 186 meaſures were expanded, by 130 ſhocks, to 211; that is, the gas was increaſed to about 1-8th its bulk.

Exper. 11. Of the ſame gas, 84 meaſures were mixed with 116 of muriatic acid gas, dried by muriat of lime. By 120 ſhocks, the mixture was a little dilated. After the admiſſion of a drop or two of water, there remained 91 meaſures; *i. e.* the addition of permanent gas was 7 meaſures, or about as much as might have been expected from the muriatic gas alone.

Exper. 12. Eighty-three meaſures of dry carbonated hydrogenous gas, with 89 of muriatic acid gas, received 200 ſhocks. The permanent reſidue, after the admiſſion of water, was 101 meaſures: the addition, therefore, amounted to 18. Of the added 18, ſix may be accounted for by the decompoſition of the water of the muriatic gas, and 10 by that of the carbonated hydrogenous gas. There remain, therefore, only two meaſures that can be ſuppoſed to be produced from the muriatic acid gas; a quantity too ſmall to afford grounds for ſuppoſing them to ariſe from decompoſed acid.

Exper. 13. Dry carbonated hydrogenous gas 132 meaſures,
mixed with dry muriatic gas - 108

240

by 200 ſhocks, expanded to - 268

Part of this gas was then transferred to another tube, and the proportion of permanent gas aſcertained. Through the remainder, 150 additional ſhocks were paſſed, before the amount of the gas thus evolved was determined. In both, it bore exactly the ſame proportion to the original gas; which ſhows that, by continuing the electriſation, no further effects were produced.

[To be continued.]

IV. *New Proceſs for Tinning Copper and other Veſſels in a durable Manner.* *By* M. BUSCHENDORF, *of Leipſic*[*].

THAT copper and braſs veſſels cannot be uſed with ſafety in cooking victuals or for holding articles of food, and particularly thoſe which contain acids, is well known. It is alſo well known that the tinning applied in the uſual manner is not durable, being ſoon worn away by cleaning, and on that account muſt be frequently renewed. Some, therefore, have propoſed enamelling for kitchen utenſils of copper; which, indeed, would anſwer exceedingly well, and be much ſafer for the health than impure tin mixed with lead, often employed for tinning: but, unfortunately, enamel is too dear, and readily breaks when the veſſel receives the leaſt blow; which cannot always be avoided[†].

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[*] From *Journal fur Fabrik, Manufactur, und Handlung*. October 1799.

[†] Articles that would come high when made ſingly, may be afforded at a low rate when manufactured on an extenſive ſcale. Cooking utenſils lined with a vitrified glazing, are now commonly ſold in many ſhops in London, and at a moderate price. It would be as reaſonable to object to the uſe of earthen-ware or china, becauſe they may be broken by blows, as to make this an objection againſt the uſe of glazed kettles.—EDIT.

The following proceſs for tinning is attended with no danger from poiſonous ingredients, as no lead is uſed in it; the tinning, too, is exceedingly durable, adds ſtrength to the copper veſſel, and ſecures it from the action of acids much longer than the common tinning:—When the veſſel has been prepared and cleaned in the uſual manner, it muſt be roughened on the inſide by being beat on a rough anvil, in order that the tinning may hold better, and be more intimately connected with the copper. The proceſs of tinning muſt then be begun with perfectly pure grained tin, having an addition of ſal-ammoniac inſtead of the common colophonium. Over this tinning, which muſt cover the copper in an even and uniform manner throughout, a ſecond harder coat muſt be applied, as the firſt forms only a kind of medium for connecting the ſecond with the copper. For this ſecond tinning you employ pure grained tin mixed with zinc in the proportion of two to three, which muſt be applied alſo with ſal-ammoniac ſmooth and even, ſo that the lower ſtratum may be entirely covered with it.

This coating, which, by the addition of the zinc, becomes pretty hard and ſolid, is then to be hammered with a ſmoothing-hammer, after it has been properly rubbed and ſcowered with chalk and water, by which means it becomes more ſolid, and acquires a ſmooth compact ſurface.

Veſſels and utenſils may be tinned in this manner on both ſides. In this caſe, after being expoſed to a ſufficient heat, they muſt be dipped in the fluid tin, by which means both ſides will be tinned at the ſame time.

As this tinning is exceedingly durable, and has a beautiful colour, which it always retains, it may be employed for various kinds of metal inſtruments and veſſels which it may be neceſſary to ſecure from ruſt.

Another durable, though ſomewhat expenſive, Method of Tinning.

This tinning, which conſiſts of more articles and is dearer than the former, can be applied to metals and metallic mixtures, and, when well prepared, is exceedingly durable; which makes up, in ſome meaſure, for the coſt. It is as follows:—

Take pure grained tin one pound, good malleable iron one ounce and a half, platina one dram, ſilver twenty-four grains, and gold three grains. Theſe five metals muſt be well fuſed together in a crucible with one ounce of pounded borax and two ounces of pounded glaſs, and the liquid matter muſt be formed into ſmall ingots. Theſe ingots are to be again heated and reduced to powder in a warm mortar with a hot peſtle. This powder is then to be put in an iron pan over the fire, where it muſt be again fuſed, ſtirring it well round: it is then to be poured into ſmall flat moulds, where it is ſuffered to cool, and it is then fit for uſe.

This tinning is to be applied in the following manner:—Firſt tin the veſſel with grained tin and ſal-ammoniac in the common manner; clean and ſcower this coating; then apply the compoſition with ſal-ammoniac according to the uſual proceſs, and when it is well diffuſed ſuffer the veſſel and the tinning to cool. Then expoſe it every where to a gentle heat to render the adheſion ſtronger, and immerſe it while hot into cold water to give it that hardneſs and ſolidity which it had loſt by being heated. The ſurface is ſomewhat rough and gritty: but you then rub it hard with a ſcratch-bruſh; and, in order to make it even, you afterwards ſmooth it completely with fine ſand or any poliſhing ingredient.

If one coating does not appear ſufficient, a ſecond, and even a third, may be applied in the manner above deſcribed. Two coatings, however, are fully ſufficient for kitchen utenſils which have been a good deal uſed; and, if you wiſh to have the ſurface perfectly ſmooth and even, you may apply a thin coating of tin, which will fill up all the cavities and render it quite even. A coal fire is the beſt for this tinning; for turf coals attack the metal, and interrupt the union and fuſion of the coating.

V. *Remarks on Dr.* GIRTANNER's *Memoir reſpecting the Queſtion, whether Azot be a ſimple or a compound Body.* *By* C. BERTHOLLET *.

DR. PRIESTLEY long ago made experiments which at firſt induced him to believe, that water diſtilled from lime, and, above all, from argil, or even without addition in an earthen retort, or one of rough glaſs, would be converted into air; but he afterwards proved that this phenomenon was fallacious, and that a tranſmiſſion only of the ſurrounding air took place on this occaſion. Guyton added ſome further obſervations to thoſe of Prieſtley †, and the circumſtance ſeemed to be ſufficiently cleared up.

Wiegleb, however, publiſhed a memoir in which he pretended to prove, by his own experiments and thoſe of ſome others, that the gas obtained by making water paſs through ignited earthen tubes, ought to be aſcribed only to the combination of the aqueous vapour with the matter of heat, and that this combination gave birth to azotic gas.

This memoir gave riſe to an anſwer publiſhed by Deiman, Van Trooſtwyk, and Lauwrenburg. Theſe learned chemiſts examined, with the greateſt care, all the circumſtances reſpecting the pretended production of azotic gas by water; and I ſhould have believed that no further doubt could exiſt reſpecting the conſequence they deduced from their experiments, *viz.* " that the azotic gas, obtained in ſome caſes by making the ſteam of water paſs through ignited tubes, aroſe merely from the exterior air deprived of its oxygen gas by the fire in which the tubes are placed, and that thus the pretended converſion of water into azotic gas, by its combination with the matter of heat, vaniſhes."

After all theſe diſcuſſions, Girtanner ſupports the new ideas which he brings forwards in his memoir ‡ on ſeveral facts, the greater part of them borrowed, and already diſcuſſed with care. Among thoſe which he affirms, we ought to conſider the following as his fundamental one: "When water is boiled in a retort of glaſs, or any other material, you obtain azotic gas." The author preſcribes theſe precautions:—"To obtain azotic gas in the greateſt quantity, the water muſt be evaporated only very ſlowly and by a very gentle heat, which you muſt take care not to augment too much."

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* From *Annales de Chimie*, No. 103.

† Encyclop. Vol. I. p. 674.

‡ See this Memoir, p. 335.

"It may be obſerved in all theſe experiments, without exception, that, when the laſt drop of water is evaporated, the azotic gas ceaſes to be produced, though the fire be continued."

An aſſertion ſo poſitive induced me to repeat the experiment with the precautions preſcribed by Girtanner. It was made with water recently diſtilled, and the precipitate from ſulphat of alum by potaſh well waſhed; it was made alſo with very white argil furniſhed by Guyton: but though the quantity of water was conſiderable, and theſe two experiments laſted, therefore, a long time, no gas was diſengaged, and the reſult was the ſame as that of the Dutch chemiſts.

It was on this fact, however, ſo eaſy to be aſcertained, that Girtanner chiefly eſtabliſhed the compoſition of azot; and hence that of the atmoſphere, "which (ſays he) is not, as hitherto believed, a mixture of oxygen gas and azotic gas, but rather a mixture of oxygen gas and hydrogen; a water in the form of gas, if I may be allowed the expreſſion. When, by chemical experiments called improperly *eudiometric*, the oxygen is ſeparated from the hydrogen, this ſeparation can never be entirely and completely effected; a part of the oxygen remains united to the hydrogen, and forms that chemical combination which we call azot, and which we obtain in theſe experiments."

Thus, when we make a mixture of oxygen gas and hydrogen gas, we form atmoſpheric air; and the differences of ſpecific gravity, of properties found by all the teſts, of products in combuſtion, are only trifling circumſtances, to which Girtanner pays little attention.

C. Bouilion-Lagrange, in conſequence of Girtanner's memoir, has made experiments more numerous than mine, but which conduct to the ſame reſults. I ſhall here give a ſhort view of them, which he was ſo kind as to communicate to me.

He did not obtain azotic gas by boiling diſtilled water alone, or with argil or alumine, in a glaſs retort, having adapted to it a glaſs or porcelain tube.

He had no diſengagement of azotic gas when he made the ſame experiment with ſilex obtained from the fluoric acid.

Having placed lime obtained from white marble in a ſimilar tube, he cauſed the ſteam of water to paſs through the tube, the other end of which was immerſed in lime-water. There was ſtill diſengaged a little carbonic acid, which formed carbonat of lime, but there was no azotic gas.

If, inſtead of lime, argil be uſed with the ſame apparatus, there is formed a little carbonat of lime, but there is no diſengagement of azotic gas.

If water be made to paſs through a porcelain tube into which lead has been put, this metal paſſes partly to the ſtate of a yellow oxyd and becomes vitrified, but no azotic gas is diſengaged.

If, inſtead of lead, you put tin into the tube, that metal becomes oxydated, and you obtain hydrogen gas. Zinc gives the ſame reſult.

To aſcertain whether this hydrogen gas contained azotic gas, C. Bouillon-Lagrange made it paſs over ſulphur in a ſtate of fuſion, and obtained ſulphurated hydrogen gas, but no azot. He ſtill ſubjected it to another trial.

He mixed the hydrogen gas he had obtained by the means of zinc with oxygen gas; and, having inflamed the mixture by an electric ſpark, water was formed, but there was no azotic gas.

Girtanner's opinion is connected with that of Humboldt on the abſorption of oxygen by the ſimple earths, and particularly by alumin. There is this difference, however: Humboldt conſiders the ſuppoſed phenomenon as a ſimple ſeparation of the oxygen, which becomes fixed, from the azot which remains in a gaſeous ſtate: but, according to Girtanner, "the azot obtained by the experiments being always a product of the operation, and not having exiſted before the experiment under the form of azot in the air examined; Humboldt, who is fond of drawing general concluſions from ſingle facts, ſeems to have been deceived when he advances that earths might be employed to determine the quantity of azot contained in atmoſpheric air: earths do not indicate the azot contained in the atmoſpheric air, they change that air into azot."

Young Sauſſure * has formally contradicted the reſult announced by Humboldt. He agrees that the *humus*, which is the reſult of a mixture of decompoſed vegetables with others not yet decompoſed, abſorbs oxygen gas, and is a fact well known; but he atteſts that "this effect does not take place when the earths are pure, and deprived of all vegetable ſubſtance." He deſcribes ſeveral experiments which he made with alumin, calcareous earth, and ſilex.

In the *Journal de Phyſique* for Pluvioſe the ſame year, there is an anſwer of Humboldt, written in a magiſterial tone, which contains nearly the aſſertions contained in the firſt memoir, with a kind of atteſtation, becauſe he made his obſervations in the laboratories of Vauquelin and Fourcroy. This authority would be impoſing, if theſe two learned chemiſts had co-operated in the obſervations; but ſo far from this, we ſee by the ſtatement that the only experiments tried in their preſence would not ſucceed.

I have been aſſured that the celebrated Fabbroni, of Florence, repeated the experiments of Humboldt at the ſame period without ſucceſs.

Champy the ſon, a very correct obſerver, repeated them at Cairo on alumin, on lime, and on the mud of the Nile, at a temperature which varied from nearly 30 to 36 degrees of the centigrade thermometer, but he obtained no abſorption: it is to be remarked that the mud of the Nile contains ſome remains of vegetable ſubſtances, ſo that when diſtilled there is extracted from it a certain quantity of carbonic acid and carbonated hydrogen gas.

Chaptal repeated them at Montpellier without obtaining an abſorption.

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* *Journal de Phyſique*, Frimaire, an. 7.

I kept moiſtened alumine a long time in contact with atmoſpheric air and with oxygen gas, without obſerving the leaſt abſorption. A chemiſt having related that the abſorption took place by the means of agitation, agitation was employed with much patience, but the reſult was the ſame. The operation was repeated with white clay which had been ſent to me by Guyton, but ſtill without abſorption. This cargil, however, expoſed to a ſtrong heat, gave a little carbonic acid and a ſmall quantity of carbonated hydrogen gas.

VI. *Deſcription of an improved Apparatus for Diſtilling, by which a conſiderable Saving may be made in the Article of Fuel; and of an Apparatus for the Rectification of Spirit of Wine.* By J. W. C. Fischer *.

THE following apparatus, if not entirely new, is attended with ſuch conſiderable advantage in regard to the ſaving of fuel, as muſt be acknowledged by every impartial and competent judge. The principal improvements may be found in Göttling's Pocket-book † for Chemiſts and Apothecaries, 1798; but, as far as I know, little or no uſe has hitherto been made of them. The reaſon of this may be, that the above work comes very ſeldom into the hands of thoſe who have moſt occaſion to make uſe of it. Diſtillers by profeſſion continue, for the moſt part, thoſe modes of operation which they have been taught in learning their buſineſs, partly becauſe they have no real opportunity of introducing improvements into their proceſs, and partly becauſe many of them, from a ſtrong propenſity to oppoſe all innovations, adhere to their old practices without taking the trouble to examine the advantage or diſadvantage of the improvements propoſed *.

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* From *Journal für Fabrik, Manufactur*, &c. December 1799.

† According to the above pocket-book, this diſtilling apparatus was firſt made in Denmark, where it has been uſed above fifty times on a large ſcale in one of the diſtilleries. The perſon who employed it got the firſt idea of it from a Daniſh journal, the author of which had copied it from a German work entitled *Auserleſene Sammlung Oekonomiſcher Schriften*, Dreſden 1796. The whole apparatus, the cooler excepted, coſt in Denmark, where every thing is dearer than in Germany, no more than 17 or 18 rix-dollars; whereas a large ſtill would coſt from four to five hundred rix-dollars. An apparatus which can be purchaſed at ſo cheap a rate, and by which one-half is ſaved in the article of fuel, muſt no doubt be of importance to thoſe who may have occaſion to employ it.

If I am not miſtaken, this apparatus originated in Denmark, where it has been employed with great ſucceſs: I have, however, ventured to introduce an improvement, the advantage of which, I flatter myſelf, cannot be conteſted. That this apparatus is not without defects, and conſiderable defects, I will readily admit; and theſe I ſhall explain, as well as the advantages: but many of theſe defects attend the common apparatus now employed, and, in all probability, may be obviated by experience and attention.

At the ſame time I have made an application of this method to the apparatus employed for the firſt and ſecond rectification of ſpirit of wine. Demachy long ago propoſed the water-bath for the rectification of ſpirit of wine; but, however convenient the apparatus propoſed by him for this purpoſe, it is attended with conſiderable expenſe, and the ſaving of fuel, which is of ſo much importance, has not been taken into conſideration.

Fig. 1. (Plate VI.) repreſents the common apparatus for diſtilling. A is a large wooden veſſel bound round with iron, into which the liquid to be diſtilled is introduced, and which may be of any ſize at pleaſure.

A hole is made in the bottom of the veſſel which occupies about a fourth part of its ſurface. In this opening, which ſerves as an air-hole, the furnace *b* is placed. This furnace may be made with moſt advantage of ſtrong copper, as iron would be ſoon ruſted by the ſurrounding fluid, and therefore would not laſt half ſo long as copper. This furnace is in nothing different from a common wind-furnace, only that the upper part of it, which in the former is open for the convenience of the workmen, is here ſhut, and the aperture for introducing the fuel is lengthened.

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* The account we gave in a former Number of the improvements introduced by the Scotch diſtillers, proves that this remark is not applicable to the whole trade: it is to be obſerved, however, that the Scotch aimed at ſaving time and exciſe duties, while the intention of the apparatus deſcribed in this paper is to ſave fuel.—Edit.

c is the tube deſtined for introducing the fuel: it paſſes through the ſide of the veſſel, and can be ſhut by the door *d*, in order to afford an uniform paſſage to the heat through the draught tubes.

It is neceſſary that the tube deſtined for introducing the fuel ſhould have the ſame height and breadth as the fire-place *e*, and it will be better that the whole furnace ſhould be rather ſquare than round, as the fire by theſe means can be better managed; and when the tube is ſhut by the door *d*, no unneceſſary eſcape of heat, notwithſtanding its width, is to be apprehended.

f is the aſh-hole, which is deſtined not only to receive the aſhes that fall from the grate, but to favour the acceſs of the atmoſpheric air neceſſary for maintaining the fire. It is therefore requiſite that the veſſel ſhould be placed on two blocks, that the air may find a paſſage under the bottom. It is of great importance that the lower part of the furnace be well joined to the bottom of the veſſel, to guard againſt the fluid running out.

From the upper part of the furnace a draught tube, *g*, proceeds alſo through the ſide of the veſſel. This tube, according to the ſize of the apparatus, muſt project from it from two to ſix inches. Above this tube there is a ſecond draught tube, *h*, which, by the tube *i*, having a right-angled knee at each end, communicates with the tube *g*, while the other end of it is deſtined to convey off the ſmoke and foul air. According to the ſize of the veſſel, this tube is at the diſtance of from ſix inches to a foot above the tube *g*. If it be neceſſary that the furnace ſhould be well faſtened to the bottom of the veſſel, it is no leſs neceſſary that the parts where the tubes paſs through the ſides of the veſſel ſhould be well joined and completely water-tight, in order to prevent the fluid from oozing through.

Around the upper edge of the veſſel there is a copper ring from three to four inches in breadth, in order to receive the copper ſhoulder *l*, which, as well as the cover, may be rendered air-tight by means of paper or linen rags daubed over with paſte. In a common ſtill, this part is not ſeparated from the body, but makes with it a whole: but here this advantage is obtained, that the veſſel, by removing the ſhoulder, may be much eaſier cleaned than the common ſtill.

It will be of conſiderable advantage if the upper aperture of the ſhoulder be equal in breadth to two-thirds of the breadth of the veſſel; and the circumference of the head, *m*, muſt be little inferior to that of the veſſel. The vapours thereby eſcape more eaſily; by which means the diſtillation is much promoted. The neceſſity of the gutter which goes round the top is evident, and in moſt apparatuſes it is employed *.

It is neceſſary that the tube *n* ſhould at the upper end be as wide as poſſible. In moſt diſtilleries it is a common fault that this tube is too narrow. By this fault the free eſcape of the vapour is impeded, and the diſtillation not a little retarded; whereas, when the tube is ſufficiently large, half the time will be ſufficient for the operation.

As the condenſation of the ſteam, particularly in large diſtillations, is not completely effected by a ſimple tube proceeding through the cooler, particularly in diſtilling on a large ſcale, where the neceſſary degree of heat cannot be always accurately maintained, ſpiral tubes have hitherto been employed; which, indeed, anſwer the purpoſe, but, on account of their expenſe and the difficulty of cleaning them, cannot always be employed with convenience. For this reaſon, I conceived the idea of an apparatus by which a conſiderable ſurface is preſented to the ſteam for cooling it, without being attended with the above inconveniences.

This apparatus conſiſts of a copper box, open at both ends, from one foot to a foot and a half in breadth, and from two to three inches in height, lying on its flat ſide. It is tinned on the inſide, and to both ends of it are applied cloſe covers, from the middle of each of which a tube projects; the one, *o*, in the upper cover, is deſtined for receiving the beak of the ſtill. From the under one the condenſed vapour flows into the lower veſſel. The copper box *p* paſſes through the oblique-lying caſk or veſſel *q*, and projects ſome inches beyond both its ends, in order to receive the covers. The box may be cleaned without any difficulty: the expenſe of it will not be one-half that of a worm, and yet it will anſwer the purpoſe of the latter completely.

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* There is no need for a head upon any ſtill: it only tends to promote a condenſation of the vapour, and its falling back into the body of the ſtill. A flat cover is all that is neceſſary. The vapour then, by its continued elaſtic force, finds its way into the worm, and is condenſed in its deſcent. The ſpace between the ſurface of the liquor and the cover ſhould, however, be ſufficient to prevent the contents from boiling up into the worm.—EDIT.

When you proceed to diſtillation, the veſſel may be filled half a foot above the upper tube *h*, and at the ſame time that tube may be connected with the tube *i*, by ſlipping their ends into each other. The fire made in the furnace will then exerciſe an equal action on all ſides: the heat proceeds through the tube *g* into the upper draught tube *h*; both tubes, as well as the furnace, are completely ſurrounded by the fluid; and the advantage of this apparatus in regard to the ſaving of fuel may be readily conceived, as that in the furnace can have no action but on the fluid, and as the heat which paſſes through the tubes is applied to the ſame purpoſe. If we calculate, on the other hand, how much fuel is neceſſary before the thick brickwork of common ſtills can be ſufficiently heated, and how much heat is loſt without being able to exerciſe any action on the ſtill itſelf, the advantage of this apparatus will be placed beyond all doubt.

If ſo much of the diſtilled liquor has paſſed over as to convince one that the tube *h* can no longer be completely ſurrounded with the liquid, but that it muſt lie above it; in that caſe it will be neceſſary, in order that the aſcending vapour may not be dilated too much by ſuperfluous heat, to remove the connecting tube *i*, by which means the communication between the upper tube and the furnace is intercepted.

One defect, which this apparatus has in common with the uſual ſtill, is, that the grains and ſlimy parts burn very eaſily in the diſtillation of ſpirits from corn; yet one might think that this fault would not take place in this apparatus to the ſame extent as in the common way of applying the fire. The grains, on account of their gravity, ſink to the bottom, on which, in the common ſtill, the fire can exerciſe the ſtrongeſt action; whereas in this ſtill the bottom of the veſſel is expoſed to the leaſt heat, and, conſequently, is better ſecured from burning. It however cannot be denied, that by the heat of the furnace the burning ſometimes may take place, eſpecially when ſufficient caution is not obſerved in managing the fire. Might not this fault be remedied by coating the furnace with a ſlight covering of ſlightly burnt clay or of plaſter or gypſum, which, as they can hold a great deal of water, would guard againſt burning without interrupting much the action of the heat on the fluid?

Rectification of Spirit of Wine by the Water-Bath.

The apparatus deſtined for this purpoſe is almoſt the ſame in the principal parts as the above, only that during diſtillation the fire cannot act immediately on the fluid, but the fluid receives the heat neceſſary for its converſion into ſteam through the medium of water brought to a ſtate of ebullition by the fire. As, in diſtilling ſpirit of wine made from corn ſpirit, the degree of heat cannot be ſo accurately graduated as to prevent the riſing of aqueous vapour, which again renders the diſtilled liquor impure, the advantageous employment of the water-bath was long ago thought of, by which the higheſt poſſible degree of heat ſhort of that of boiling water can be obtained. Demachy propoſed for this purpoſe a copper kettle filled with water, in which the ſtill was ſuſpended. But it may be readily ſeen, that a much larger furnace than common would be neceſſary for this purpoſe, and conſequently a larger proportion of fuel; I ſhall therefore deſcribe an apparatus deſtined chiefly to promote a ſaving of fuel, and which, I am perſuaded, is in nothing inferior to that propoſed by Demachy.

Fig. 2. repreſents the apparatus without the cooling part, which is entirely the ſame as that already deſcribed for ſimple diſtillation. A is a wooden veſſel bound round with iron hoops, to the bottom of which is faſtened the before-deſcribed furnace *e*, with the aſh-hole *f*, and the tube which ſerves for introducing the fuel, only that it is not applied to the middle but to the ſide of the veſſel. The tube *g g*, deſtined to afford a paſſage to the air, and to convey off the ſmoke, does not conſiſt, as in the former apparatus, of three pieces, but of one whole, which does not paſs directly through the ſide oppoſite to that where the fuel is ſupplied, but extends at moſt only to two-thirds of the width of the veſſel, where, by means of a knee, it turns up, and, by a ſecond knee, is brought back to the other ſide. The whole furnace occupies, in general, only one-half of the bottom of the veſſel, and is equal in height to only one-fourth of that of the veſſel.

Over the furnace there is a copper box *b*, which occupies nearly two-thirds of the diameter of the veſſel, as if in a ſemicircle, and completely covers the furnace as well as the tubes. Its height is about two-ſevenths of that of the veſſel. Strictly ſpeaking, it has only one ſide; for the reſt are formed by the ſides of the veſſel. From the upper part of it there proceeds a ſmaller longiſh box, *c*, which paſſes through the ſide of the veſſel, and the upper part of which above the veſſel is open. In the laſt place, on that part of the bottom of the veſſel covered by the box there is a cock, *b*, which ſerves for conveying off the water. The remaining parts of the apparatus are not different from thoſe of that above deſcribed.

When you proceed to diſtillation, the box is completely filled with water through the part that protrudes beyond the veſſel; and when the water has been brought to a ſtate of ebullition by the furnace, it can communicate to the ſpirit of wine the heat neceſſary for converting it into vapour. If you are deſirous, however, to carry on the operation with as much accuracy as poſſible, it will be neceſſary to introduce a thermometer into the box *c*, which is open without; for, as the ſpirit of wine boils at 165 degrees of Fahrenheit, and water requires 212, the heat of the boiling water will be ſufficient to raiſe the watery particles, though the fluid in the veſſel will not acquire that temperature. It will be moſt advantageous to maintain the heat till the thermometer indicates from 170 to 175 degrees. If this degree of heat be continued without ſuffering it, by careleſſneſs, to be raiſed higher, the ſpirit of wine, by the firſt rectification, will be pretty pure, and free from water; but it is neceſſary, towards the end of the diſtillation, to ſtrengthen a little the degree of heat, as, without this precaution, the laſt ſpirit of wine that paſſes over would be rendered impure by more or leſs water.

In common, corn ſpirit has an unpleaſant ſmell and taſte, the primary cauſe of which is careleſs management of the fire during the firſt diſtillation as well as during the rectification; as by theſe means, in the firſt diſtillation in particular, the grains readily become burnt, and conſequently communicate that taſte and ſmell to the diſtilled liquor. The decompoſition of the gelatinous matter of the grain ſeems alſo to contribute to this defect. The addition of lixivious ſalts, chalk, or charcoal, during the rectification, is the beſt means to improve ſpirits which have been hurt in this manner.

I am however of opinion, that it is poſſible to preſerve the corn ſpirit in a perfect ſtate during the firſt diſtillation. The fault generally lies in the fermentation of the waſh being too long continued. In general, the diſtillation is not begun till the fermenting liquor throws up no more bubbles, and ſhows itſelf perfectly tranſparent under the ſcum: but it has been confirmed by ſeveral experiments, that the corn ſpirit is obtained ſtrongeſt, and in the greateſt abundance, when the mixture is ſubjected to diſtillation in its higheſt degree of fermentation. If the fermentation be ſuffered to obtain its utmoſt term completely, a quantity of vinegar will be formed, which not only contributes a great deal to give the liquor an unpleaſant taſte, but actually occaſions a conſiderable loſs of the ſaccharine matter neceſſary for the production of ſpirit of wine.

VII. *On the Preparation of Amber Varniſh, and the Application of it to different Kinds of ſtained Wood.* *By* NILS NYSTRÖM, *Apothecary of Norrköping* *.

AS furniture of foreign wood is in general expenſive, the uſe of the indigenous kinds of wood ought not to be neglected, eſpecially when they are of a compact texture, have a fine grain, and are ſufficiently hard. Furniture made of theſe kinds of wood, after it has been well poliſhed, may be ſtained of different colours, and then done over with linſeed oil and amber varniſh.

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* From the *Tranſactions of the Academy of Sciences at Stockholm for the Year* 1797.

As my principal view, however, was to try in what manner different kinds of wood could be ſtained ſo as to retain their colour longeſt, and in what manner mahogany could be beſt imitated, I applied ſuch a compoſition as I thought would beſt anſwer the purpoſe on the twelve following kinds of wood; *viz.* elm, oak, red and white beech, maple, pear-tree, wild hawthorn, white beam-tree, aſh, alder, birch, and pine. Of theſe, the maple, birch, alder, and white beech, when ſtained with a ſolution of iron, had the greateſt reſemblance to mahogany. The appearance of the other kinds was various, according to the diverſity of their colour and veins, and according as they were more or leſs porous, and imbibed a greater or leſs quantity of the ſtain.

The amber varniſh prepared in different places is not always of equal goodneſs. This is owing to two cauſes: 1ſt, The careleſs manner in which the amber is melted; 2d, Becauſe the linſeed oil, being too much or too little boiled, acquires, in a greater or leſs degree, a drying property. A proof of amber varniſh being good, is, if, when applied on any article in an apartment of the common temperature, it becomes dry within from twelve to twenty-four hours.

By the experiments which I made, I have found that the following proceſs for preparing it is the beſt:—Put pounded and ſifted amber into a pan of caſt iron with a flat bottom, and let the amber be ſpread out at moſt to the thickneſs of an inch. Place the pan in an upright poſition over an uniform coal fire, and let it remain till the amber fuſes and becomes liquid; then pour it out on a cold plate of copper or iron. When the amber has cooled, break it into pieces; and if it has a bright blackiſh-brown appearance on the fracture, and weighs about one-half of what it did before being melted, you may be certain that it is proper for the intended purpoſe. Particular attention muſt be paid to this circumſtance; for, if the amber is fuſed too little, ſo that part of it remains unmelted, it will not be diſſolved by the varniſh; and if the operation be performed with a continued and ſtrong heat, it will be burnt and unfit for uſe.

When the amber is kept over a weak fire it will not melt, but becomes a ſort of blackiſh brown incruſtation, which alſo may be employed for varniſh provided it has not been too much or too little evaporated: it will be in the beſt condition when you obtain half a pound from a pound. If the amber is too little evaporated, it muſt be again put into the pan till it be reduced to the proper weight. The ſame thing is to be obſerved when you melt it; but the parts which are not fuſed muſt be picked out, in order to be afterwards alſo melted.

I have found that a pan with a flat bottom is better than one with a round bottom, becauſe the melting or evaporation is effected ſooner in the former than in the latter; for in the latter the amber lies thick in the middle, and is burnt at the bottom and ſides before it can be brought to melt or evaporate.

It is not neceſſary, for making varniſh, to pick out pure and tranſparent amber, but only the common yellow ſmall fragments, which may be procured for half the price of that in lumps. The earthy part, which is found in amber not of the cleareſt kind, ſeparates itſelf from the warm varniſh when it is ſuffered to ſtand ſome time before it is decanted.

Method of preparing Linſeed-Oil Varniſh.

One pound of well pulveriſed and ſifted litharge, four ounces of finely pounded white vitriol, and one quart of linſeed oil. Put theſe ingredients into an iron pan of ſuch a ſize that it may be only half full; mix them well together, and boil them till all the moiſture is evaporated, which may be known by a pellicle being formed on the ſurface, or by the barrel of a quill burſting when thruſt to the bottom of the boiling varniſh. Then take it from the fire and pour off the clear liquid, taking care to keep back the thick part which has depoſited itſelf at the bottom. While boiling, it muſt be ſtirred ſeveral times round, that the litharge may not fall to the bottom; but you muſt not ſtir it conſtantly, elſe ſuperfluous litharge would be diſſolved, and the varniſh become too thick.

The compoſition of amber varniſh conſiſts of half a pound of melted or roaſted amber, one pound and a half of linſeed-oil varniſh, and two pounds of turpentine oil. The amber and linſeed-oil varniſh are to be mixed together in a deep caſt-iron pan, of ſuch a ſize as to be only one-third full, and to be kept over a ſlow fire till the amber is diſſolved, which may be known by its ſwelling up: the operator, therefore, muſt have at hand a large copper or iron veſſel, that the varniſh may be held over it in caſe it ſhould riſe above the ſides of the pan, and to prevent the loſs that would thereby be occaſioned. When the varniſh is diſſolved, the pan muſt be taken from the fire; and when the mixture has cooled, the turpentine oil is to be poured into it, continually ſtirring it. Then let it ſtand ſome time, that the coarſe undiſſolved particles may depoſit themſelves at the bottom; after which pour off the clear varniſh, and, having ſtrained it through a piece of linen, put it into bottles for uſe.

In boiling the varniſh, care muſt be taken that it may not boil over or catch fire. Should this happen to be the caſe, it muſt not be extinguiſhed by water; for this mode would occaſion ſuch a ſputtering, that the operator would be in danger of having his face beſpattered with the boiling varniſh. The beſt method, therefore, is to cover the veſſel in ſuch a manner as to exclude the air, and with any thing that may be at hand, ſuch as a piece of wood, plate of iron, or any thing elſe that may cover the veſſel and extinguiſh the flame.

1. *Iron Stain.*

Eight ounces of iron-filings and thirty-two ounces of common aquafortis. The aquafortis muſt firſt be mixed with ſixteen ounces of water in a ſtone jar, and then a few of the filings are to be added, and well ſtirred round with an iron or wooden ſpatula. This preparation muſt be made in a chimney, becauſe the ſolution is attended with heat, effervеſcence, and the diſengagement of noxious vapours: it is of importance, alſo, that the jar ſhould be of ſuch a ſize as to prevent the matter from running over. After the greater part of the iron has been diſſolved in this manner, the ſolution will be of a yellowiſh brown colour. As ſoon as the mixture has cooled, pour it into a bottle placed in a pan, and let it ſtand a day or two, without being corked, over a warm ſtove; during which time ſhake the bottle frequently. The bottle being then ſuffered to cool, pour into it one pound three quarters of river water, ſtirring it well round, and leave it at reſt for a few minutes till the undiſſolved part ſinks to the bottom: then pour the ſolution into another bottle, and cork it up for uſe.

When this preparation has been carefully made according to the above directions, you obtain a yellowiſh brown ſolution, which may be employed for ſtaining. In caſe too much water, however, has been at firſt employed, or too weak aquafortis, the colour becomes dark brown; and therefore the addition of the aquafortis muſt be increaſed in that proportion which the ſolution requires, obſerving the above rules.

This ſtain may be applied to all kinds of wood except oak, which contains too much aſtringent matter, and therefore on the firſt application becomes almoſt black. It communicates to the various kinds of wood different colours, according as it is applied in greater or leſs quantity; ſuch as yellow, yellowiſh brown, and dark brown, with reddiſh brown ſtripes or ſpots.

As far as I have been able to aſcertain by experience, this ſtain is one of the moſt durable; it withſtands the air and rays of the ſun without changing its colour. I have ſpecimens of it which have been expoſed four years continually to the ſun and air, without their colour being altered.

2. *Brazil Wood Stain.*

Eight ounces of real Brazil wood, four ounces of alum, and four ounces of finely pounded reddle or red ochre. Pour over this mixture a proper quantity of water; and, having ſuffered it to remain in that ſtate twenty-four hours, boil it to a fourth part; then ſtrain it through a piece of linen cloth, and preſerve it in a glaſs bottle for uſe.

If one pound of the ſtain No. 1. be diluted with three pounds of water, immerſe ſome pieces of wood in it, and depoſit the whole in a warm place: the wood will imbibe the liquid to the depth of a quarter of an inch, and in the ſoft parts of the wood it will penetrate ſtill further.

It muſt however be remarked, that the ſtaining liquor muſt be diluted with a greater or leſs quantity of water, according as the wood is darker or whiter, more or leſs aſtringent. The wood muſt often be turned, and care muſt be taken that it may acquire an uniform and proper colour; after which it may be taken out and dried.

The like proceſs muſt be followed when the ſtain No. 2. is employed, only that the ſame attention is not neceſſary in regard to obtaining an uniformity of colour. This mode, however, can be uſed only on a ſmall ſcale.

Method of ſtaining Articles of White Wood with the above Compounds.

Rub the ſtain No. 1. over the wood with a piece of ſponge five or ſix times till it acquire a proper mahogany colour. While the liquor is applied, ſhake it or ſtir it carefully round, that the iron ochre, which has depoſited itſelf at the bottom, may be well mixed with the ferruginous ſolution that ſtands over it; and between each application of the liquid the wood muſt be ſuffered to become thoroughly dry. After this, it muſt be rubbed over once or twice with linſeed oil, letting it dry before the oil is applied the ſecond time. The more the wood is ſoaked with linſeed oil, ſo much the better; as in that caſe it does not imbibe ſo much of the amber varniſh, which only depoſits itſelf on the ſurface, and gives it a bright appearance.

When the wood has been thus rubbed over, and well dried, the amber varniſh muſt be applied in an uniform manner with a ſponge once or twice, or until it acquires a ſmooth ſhining ſurface. If the wood has been well done over with linſeed oil, one application of the varniſh only will be neceſſary; but on bedſteads, cheſts of drawers, chairs and other furniture which are expoſed to more uſe, it muſt be applied ſeveral times, and each time they muſt be well dried.

After the application of the varniſh, if any inequalities or lumps appear, they may be removed, after the article is dry,

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by means of a carpenter's ruſh, and a freſh coating of varniſh applied to the place. For applying the varniſh, I have found it moſt convenient to uſe a ſponge; as by theſe means it can be laid on in a much more uniform manner than by a painter's bruſh, which, for the moſt part, leaves ſtripes or looſe hairs behind it. The ſponge with which the varniſh is laid on, muſt, between each application, be well ſhut up in a wide-mouthed bottle, that the varniſh it has imbibed may not be dried by the air, which would render it hard, and unfit for uſe.

The ſame proceſs is to be followed with the ſtain No. 2. When applied to knotty birch wood or alder, the wood becomes undulated, becauſe the liquor extends itſelf more croſswiſe. The caſe is the ſame with fat and knotty pine wood or fir; for the reſinous ſpots do not ſo ſtrongly attract the ſtain as the other parts. This, however, makes the appearance of the wood not diſagreeable.

The amber varniſh may be applied alſo over almoſt all oil colours except blue, which it would change to green. It is attended with this advantage, that it never cracks; as is the caſe in general with lack varniſhes, and thoſe prepared with ſpirit of wine.

I have found, alſo, that this varniſh is proper for being applied on real gilding, becauſe it makes no perceptible alteration in the colour; eſpecially when laid on thin, and in an uniform manner. This object alſo is obtained, that the gilding may be cleaned by means of ſpirit of wine, or of ſoap and water, without ſuſtaining any injury. For falſe gilding, however, it is improper; as it attacks it, and makes it ruſty and green.

I have applied this varniſh, in the above manner, to bedpoſts, drawers, tables, and chairs, which at the end of two years were little or nothing changed in their appearance. When this varniſh has become perfectly dry and hard, it withſtands boiling heat and friction; and does not loſe its ſplendour by the courſe of time, unleſs rubbed or ſcoured with ſand. It prevents ink, or any other coloured liquid that may be ſpilt on furniture, from penetrating into the wood, and cauſes ſtains to be eaſily removed by waſhing with water.

On this account, it is very proper for being applied on articles of mahogany. It renders the ſoft kinds of wood much harder at the ſurface.

This varniſh uſed in this manner would not be expenſive, as eight ounces of it is ſufficient for a common card-table. Should the table be ſcratched, the injured part may be eaſily repaired by a new coating of varniſh; and if it be new varniſhed every four or five years, it will always retain a beautiful and bright appearance.

Tables and other flat articles which require a great deal of poliſhing, when the ſtain and linſeed oil have been applied, may be rubbed with a piece of pumice ſtone before they are done over with the varniſh. This pumice ſtone muſt be made flat on one ſide, and muſt be free from ſand, ſo as not to ſcratch the wood. As the ſofter kinds of wood ſwell up, for the moſt part, when the ſtain is applied, and loſe their ſmoothneſs, rubbing them in the above manner with pumice ſtone is the more neceſſary.

VIII. *Comparative View of ſome dangerous Diſeaſes, ſuppoſed to be occaſioned by Inſects, which prevail in Sweden, Ruſſia, Siberia, and the adjacent Countries.*

[Concluded from Page 144.]

THE late Dr. J. J. Lerche, of Peterſburgh, communicated to me the following account of ſome peſtilential diſeaſes which manifeſt themſelves in the marſhy diſtricts around that city and in other places during dry ſummers, and which appear to me to be of the ſame nature as the Siberian diſeaſe before mentioned. But that I may not be accuſed of endeavouring to pervert facts for the purpoſe of ſupporting my own opinions, I ſhall here give the author's own words:

" In the year 1756, a very hot dry ſummer, unhealthful for men as well as cattle, took place after a very unhealthful winter. The diſeaſe which then prevailed continued till the harveſt, and extended on one ſide to Moſcow, and on the other to Livonia and through Finland. As ſoon as the ſenate were informed of it, Condoide, phyſician to the court, was ordered to diſpatch phyſicians and ſurgeons to thoſe places where their preſence was moſt neceſſary. The chief complaint was reſpecting the mortality among the cattle; but in ſome places men and women were ſeized with the diſorder, and died ſuddenly. I was obliged, therefore, to ſet out in all haſte on the 26th of July, accompanied by Chemnitz the ſurgeon, for the village Tſchudowo, at the diſtance of 116 werſts from Peterſburgh, where we arrived next day. On inquiry, we learned from enſign Solopof, who commanded the poſt-people in that diſtrict, that, ſince the 17th of June, ſeven perſons had died, in the courſe of from two to four days, with boils and tumours, and that two were ſtill exceedingly ill: that, on account of the dryneſs of the land, ſeveral cows, but particularly horſes, had died alſo. On receiving this intelligence, we cauſed the road on each ſide of the village to be barricadoed with planks, eſtabliſhed guards at the barriers, and obliged the poſt and other travellers to paſs round over the fields to the next ſtation. We then entered the village to viſit the ſick, and found that one perſon had died in the night-time with carbuncles on the neck and legs. A woman who had a large carbuncle on her face was ſtill alive. In the courſe of about a month, four cows and twenty-eight horſes had died, all of which had tumours on the neck, breaſt, belly, or ſcrotum. I deſired an officer and twenty ſoldiers to keep ſtrict watch, in order to prevent all communication between the village and the neighbourhood; and we then took up our quarters on the rivulet called Keretz, in a houſe belonging to field-marſhal Albrecht, which was then unoccupied. In a little time ſeveral more were attacked by the like affections, and died in the courſe of one or two days. They all had tumours on their neck, breaſt, and head, with black bliſters like real carbuncles; but we obſerved in none of them either cold or heat: their pulſe was ſcarcely perceptible; they lay ſome time ſenſeleſs, and then expired.

" As the diſeaſe now began to extend itſelf to the neighbouring villages, I ſent for more ſoldiers, and the ſenate diſpatched lieutenant De Cominges with twenty men. The poſt-ſtage at the diſtance of a werſt and a half from us, where there were twenty people and thirty horſes, appeared to be in the greateſt danger. As the people and horſes began to be attacked with the like ſymptoms, and to drop off, no time was to be loſt; we therefore ordered the people and horſes to be removed to the woods, and to be cloſely guarded. Travellers were obliged to proceed, without ſtopping, to the next ſtage; and in conſequence of a repreſentation made by us on the ſubject, the whole road from Novogorod to Peterſburgh was afterwards ſtopped, ſo that travellers were forced to purſue the old road to Novogorod by Kraſnoe Selo. The preventing of all intercourſe with the ſurrounding country ſeemed the more neceſſary, as in other places, ſuch as Toſna, Luban, and from Soſninſkoi to Novogorod, ſeveral horſes and ſome men had already died. The road alſo by Iſchora and Novogorod was ſtopped; and this circumſtance occaſioned great alarm at Peterſburgh. Lieutenant Freyman, at Iſchora, ſuffered no perſon from us to approach that place; and would not receive our report, till we ſent a ſubaltern to repreſent to him the neceſſity of ſending off our information; which he at laſt did, but not till he had employed the precautions uſed in regard to letters from places where peſtilential diſeaſes prevail, ſuch as fumigating them and drawing them through vinegar.

" As all the means propoſed for checking this diſeaſe were of no avail, I at laſt conceived the idea of laying aſide all other medicines, and confining myſelf to cinchona alone. This remedy, indeed, produced an evident change; for the mortality ſoon after ceaſed. The patients acquired a ſtronger pulſe, perſpiration and evacuation by ſtool were promoted, and they ſoon recovered their appetite. The tumours in the face diſappeared, and the carbuncles dried up of themſelves. I applied to them neither ſalves nor plaſters; for I clearly ſaw that theſe tumours would not be affected by external means: veſicatories and ſcarification made them only more difficult to be healed. At length the evil ceaſed; yet care was taken to keep the diſeaſed ſeparate from the ſound, and to employ all the other precautions uſual in the time of the plague.

" On cloſer examination, I found that all thoſe attacked by the diſeaſe, without exception, had handled or touched diſeaſed cattle, or had proceeded incautiouſly in interring them: but all thoſe who had touched the cattle were not infected; for a great many horſes in all the villages, and even in Peterſburgh and on the road to Peterhof, died with the like ſymptoms. Yet in ſome of theſe places none of the people were attacked by the diſeaſe. In Tſchudow, however, twenty-five perſons were infected, of whom ten recovered. The diſeaſed never communicated the infection to any of thoſe who attended them, or lived with them; ſo that the diſeaſe could be no plague; nor were any buboes obſerved under the arm-pits, or in any of the other glandular parts. Of our people, who attended the ſick and bound up their ſores, not one was infected. The diſeaſe at length began to diſappear among the people; and about the 20th of July, when a conſiderable quantity of rain fell, and the air became cooler, very few of the cattle were any longer ſick: for this reaſon, the ſenate again opened the roads on the 1ſt of Auguſt, and cauſed the guards to be removed. Tſchudow, where the diſeaſe had ceaſed among the people about the 18th of July, continued ſtill ſhut four weeks longer; but on the 16th of Auguſt we obtained leave to return.

" In the year 1761, the heat during ſummer was exceedingly intenſe, and in the diſtricts of Narva, Novogorod, and Kexholm, had occaſioned a mortality among the cattle like that which uſually takes place during hot ſummers in the marſhy parts of Finland. Some of the people alſo were infected with exanthemata, or were attacked with carbuncles, or rather gangrenous tumours, and ſome of them died ſuddenly. As I at that time preſided over the medical department, I diſpatched, by command of the ſenate, doctors Ens and Dahl, together with ſome ſurgeons, to the place where the diſtemper was ſaid to prevail; but about the beginning of September it entirely ceaſed.

" In the month of July 1764, information was brought to the ſenate from the diſtrict of Jamburg, that ſeveral cattle had died on the Schepelow eſtate; and that one man had been attacked by the diſeaſe, and died ſuddenly. When this intelligence was communicated to the empreſs, ſhe immediately gave orders to baron Tſcherkaſſof, preſident of the college of medicine, to ſend me thither, as had been the caſe before on ſimilar occaſions. On the 29th of July the preſident made me acquainted with this order, and ſent for Chemnitz the ſurgeon. We ſet out the ſame day, with one of the medical pupils and a few ſoldiers, and in the evening of the following day arrived at Ugoric, the place of our deſtination. I ſent notice of our arrival to Kudrawzof, the commiſſary at Jamburg, who had firſt communicated intelligence of the diſeaſe having broken out, and who joined us the day following. In conſequence of falſe information given to him by a peaſant, Kudrawzof had firſt ſtated, that after twenty-four head of cattle had died, a farmer named Iwan Petrof had loſt his horſe, which had dropped down and expired as he was riding home from the fields; that the farmer had been infected with the diſeaſe, which ſhowed itſelf in livid tumours on his face and breaſt, and that he had died the third day after. We learned, however, that the caſe was ſomewhat different. Petrof had indeed loſt a horſe, which he had ſent with his ſon in the beginning of June to Narva, where the animal died: Petrof himſelf had walked out into the fields; but when he returned his eyes were ſwelled, and his whole face, breaſt, and ſhoulders, being ſoon after affected in the like manner, he died on the fifth day. From theſe ſymptoms it would appear that his diſorder was a malignant eryſipelatous ſwelling in the head, which became ſtill worſe becauſe no remedy was applied. All theſe circumſtances were told us by his wife and ſon, and confirmed by the prieſt of the place. The diſeaſe, therefore, of which he died, could not be conſidered as infectious: three children whom he left, together with their mother, were in perfect health; but we cauſed them to obſerve the neceſſary precautions for eight days.

" In regard to the diſtemper among the cattle, it began on the 15th of June in three places. Twenty-two horſes and two cows had died at Ugorie, three horſes at Letoſchez, and five ſwine at Lätzi. The ſame ſymptoms were obſerved on all theſe animals, *viz.* tumours on the neck, breaſt, and ſhoulders, or under the belly. They had all died ſuddenly, and moſt of them within twenty-four hours after the diſeaſe had made its appearance. Only two of the horſes attacked by the diſeaſe eſcaped. The peaſants had ſcarified all the tumours with a red-hot iron, but without any benefit, though that proceſs had been ſerviceable on former occaſions. The diſeaſed cattle were able to walk about till the laſt moment.

" On receiving this information I ordered the ſtaroſt to bring us the diſeaſed cattle; and we reſolved, inſtead of ſcarifying the tumours, that ſetons ſhould be put into their breaſts, that they ſhould be driven about till they perſpired, and that they ſhould then be curried and carefully rubbed down. This, however, was not attended with the deſired effect. On the 7th of Auguſt one of the horſes died in the fields with a tumour on his breaſt. But this was the laſt: the cool wet weather which now took place contributed in a great meaſure to put an end to the malady; and we returned on the 11th of Auguſt.

" The great heat of the ſummer in the year 1766 occaſioned a mortality among the cattle in ſeveral places, but chiefly in the neighbourhood of Peterſburgh. One-half of the oxen died; all of them were attacked with a kind of glanders and ſore eyes. They generally died in the courſe of three days. On account of this diſeaſe I was obliged to proceed to the eſtate of prince Orlow, at Schunderowſka Muiſa, fourteen werſts from Kraſnoi Zelo, accompanied by a ſurgeon and ſeveral ſoldiers. This place is ſituated at the diſtance of eight werſts from Goreloi Kabak, on a conſiderable eminence, from which the capital may be ſeen. I was informed by the bailiff on the eſtate, that twelve horſes had died, all with tumours on the breaſt and ſcrotum, in the courſe of a few hours. Two horſes and three cows died during my ſtay in the place. The preceding winter, the itch had attacked a great many of the horſes; all of whom the bailiff, with the count's conſent, had cauſed to be ſhot.

" On the 13th I was informed by the ſtaroſt, that at the diſtance of ſix werſts, two cows out of twelve, belonging to a farmer, had died ſuddenly without any external ſymptoms of diſeaſe, and that the farmer's wife had at the ſame time tumours on her head. Though I received this information in the evening, I immediately proceeded thither; but did not find the woman at home. Being brought to me next morning, I found that ſhe had actually a black tumour not unlike a carbuncle. We gave her the neceſſary medicines; and, as ſhe found herſelf conſiderably better, we made preparations next morning to return.

" When we got back to Goreloi Kabak, we were informed by the poſtman, who three days before had driven us through Schunderow, that the ſame day he had been attacked with a tumour above the right eye. In the middle of it there was now a black ulcer not unlike a carbuncle, but unattended with heat. I adminiſtered to him the proper medicines; but I received no further account of him, though I gave directions to the bailiff for that purpoſe."—Thus far Dr. Lerche.

I ſhall leave it to intelligent phyſicians to determine whether this diſeaſe, ſo dangerous in particular to horſes, which prevails only during dry and hot ſummers in Ingermannland and Finland, be not of the ſame kind with that obſerved in Siberia; and whether it be not free from all dangerous conſequences in regard to infection, and may be cured by the common topical means. The uſe of bark, which Dr. Lerche found beneficial, ought undoubtedly to be employed for curing this diſeaſe, and preventing the gangrene, which ſo ſuddenly takes place, from ſpreading. It is to be aſcribed, in all probability, to a like power of checking gangrene, that the Calmucs have found it of benefit to give to horſes, when attacked by theſe tumours, as much as they can eat of that ſtatice which I have called *trigona* *. They, however, aſſert too much when they ſay, that a horſe which has been once cured by this aſtringent plant will never be again attacked by the diſeaſe.

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* Pallas *Reiſe*, Part III. p. 519, note. Gmelin's *des Jüngern Reiſe*, Part II. plate 34.

IX. *Obſervations on the Tranſition of animal or abſorbing Earth to the State of calcareous Earth*[*]. *By* B. G. SAGE, *Director of the firſt School of Mines.*

THE aſhes produced by burnt bones are white, and compoſed of more than two-thirds of animal earth, and of a part of that ſame earth combined with phoſphoric acid. They produce by lixiviation a pretty large quantity of natron, from which the fire, according to all appearance, has ſeparated the phoſphoric acid.

Bone-aſhes ought to be conſidered as a phoſphoric ſalt with an exceſs of animal earth. Theſe aſhes, deprived of their natron by lixiviation, are inſipid, and inſoluble in water; but the phoſphoric ſalt becomes ſoluble if it be diſengaged from the exceſs of animal earth with which it is combined, and which may be effected by means of the vitriolic acid. Fourteen parts of this acid concentrated are requiſite for twenty-four of bone-aſhes: the more they are calcined to whiteneſs, the larger will be the quantity of vitrifiable phoſphoric acid extracted from them. It is contained in them in the proportion of a third. This ſalt, compoſed of animal earth and phoſphoric acid, may be decompoſed by fixed alkali, which precipitates from it an inſoluble calcareous phoſphorous ſalt. This character of inſolubility ſerves to ſhow that there is a difference between the animal earth and the calcareous earth, ſince the phoſphoric acid ſalt, with a baſe of animal earth, is ſoluble in water. When vitrified, it produces a pellucid maſs of a light blue tint; while the phoſphoric ſalt with a calcareous baſe produces by fuſion a ſemitranſparent white glaſs, cryſtalliſed at its ſurface into a kind of dendrites.

Six ounces of vitrifiable phoſphoric acid ſalt, dried into a ſoft paſte, required four ounces of fixed alkali of tartar to be decompoſed. The white precipitate, waſhed and dried, weighed one ounce ſix drams.

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[*] Calcareous earth differs from that of bones in being compoſed of *acidum pingue* and an exceſs of animal earth. Calcination reduces it to lime: but this is not the caſe with the earth of bones, which is a phoſphoric ſalt with exceſs of animal earth. The name of *phoſphat of lime*, given to the earth of bones, is conſequently improper.

The leys, when evaporated, produced three ounces five drams of phoſphoric tartar. This ſalt, when expoſed to heat in a crucible, liquefies, ſwells up, and becomes fuſed. If it be poured on a plate of iron, it has the tranſparency of glaſs as long as it is warm, but becomes white and opake on cooling. This ſalt when fuſed is ſapid, and ſoluble in water: it loſes by fuſion three-eighths of its water of cryſtalliſation.

In the decompoſition of the vitrifiable phoſphoric acid ſalt by fixed alkali, more than half of the alkali is decompoſed; ſince there are obtained no more than three ounces five drams of phoſphoric tartar, which contain three eighths of water.

The portion of *acidum pingue*, the principle of the fixed alkali [*], which modifies itſelf into mephitic acid gas, is very ſmall. This *acidum pingue* combines with the animal earth, and conſtitutes calcareous earth, which ſaturates itſelf with phoſphoric acid, and forms the inſoluble ſalt above mentioned [†].

X. *Account of a new, eaſy, and more convenient Proceſs for reſolving Minerals by Alkalies.* By M. LOWITZ [‡].

THE decompoſition of mineral bodies is, without doubt, one of the moſt difficult and moſt laborious operations of chemiſtry, and which muſt ſtill become more difficult and complex in proportion as we are acquainted with a greater number of new and component parts in foſſil bodies; for every new diſcovered earth or ſpecies of metal requires afterwards,

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[*] When fixed alkali is employed for the precipitation of any ſubſtance, there is always a part of the alkali decompoſed, and the acid and earth contained in it form part of the precipitate. Of this, mercury diſengaged from the nitrous acid by fixed alkali affords an inſtance. If this precipitate be diſtilled, it is ſublimated into a mercurial ſalt *ſui generis*. The earth of the alkali remains at the bottom of the retort.

[†] In the above curious paper the author's own nomenclature has been followed.—EDIT.

[‡] From Crell's *Chemical Annals*, 1799, Vol. II. part 10.

wards, in the examination of other minerals, that our operations ſhould be multiplied. As this occaſions a great conſumption of time, it may at length happen that the chemiſt, in the whole courſe of his life, eſpecially as human life is ſo ſhort in general, would not be able to examine, with the requiſite accuracy and patience, but a very ſmall number of minerals. The worſt conſequence of this would undoubtedly be, that our progreſs in the ſtudy of nature would be much retarded.

This inconvenievce, however, might be remedied, if chemiſts in their reſearches would endeavour to ſhorten and ſimplify their operations as much as poſſible; and, when they diſcover eaſier and ſhorter proceſſes, if they would make them known in a chemical journal, which might be entitled, Collections towards facilitating Chemical Decompoſitions. After this ſhort digreſſion, I ſhall now proceed to the main object of this paper, which, I hope, will be conſidered a contribution towards a work of the above kind, not unimportant, and which may be thought worthy of ſome attention.

It is well known that moſt foſſils, particularly thoſe of the earthy or ſiliceous kind, to be rendered ſoluble by acids, and conſequently fit for being analyſed, require a preparation with fixed alkalies; an operation which is called *reſolution.*

It has hitherto been believed that this reſolution could be effected only by reducing the foſſil to an exceedingly fine powder, mixing it with alkaline ſalts, and then keeping it for ſeveral hours in a ſtate of ignition. Formerly, the alkaline carbonats were made choice of for this purpoſe, till profeſſor Klaproth, ſome years ago, ſhowed that the pure or cauſtic alkalies were preferable. This meritorious chemiſt, who has contributed ſo much to the improvement of our chemical knowledge, recommends that the foſſil, finely pounded, ſhould be firſt boiled to dryneſs with cauſtic alkali, and that the dry maſs ſhould then be kept at a red heat for ſeveral hours in a ſilver crucible, taking care, however, not to melt the crucible. This method has hitherto been conſidered, both by myſelf and by other chemiſts, as the beſt, and on that account has been followed.

For ſome time paſt, however, this method of keeping the maſs in a ſtate of ignition has appeared to me objectionable; and for the following reaſons:

1ſt, It happens not unfrequently that the maſs itſelf is really fuſed, by which means the ſoftening it afterwards by water is rendered much more difficult.

2d, By the leaſt accident in regard to the proper management of the fire, there will be great danger of the crucible itſelf being fuſed.

3d, If the chemiſt, by purſuing the above proceſs, diſcovers in any foſſil a new earth or metal, it is often objected to him by other chemiſts, that the object of his ſuppoſed diſcovery may be nothing elſe than a product of ſome peculiar modifications of the component parts of the foſſil ſubjected to examination, effected by the action of caloric.

4th, As it is an acknowledged truth, eſtabliſhed from a comparative view, that the wet way deſerves, in many reſpects, to be preferred to the dry way, it muſt be highly unpleaſant on all occaſions, eſpecially in the commencement of the analyſis of a mineral body, to be obliged to proceed in the dry way.

On this account, therefore, I reſolved to make ſome experiments on the reſolution of foſſils in the wet way; and the reſult exceeded my expectation; as I was able to perform this operation (which hitherto, on account of the ſtrong fire requiſite for the purpoſe, was obliged to be performed in a laboratory, or, at leaſt, under a chimney,) not only by the help of the weak flame of a very ſimple ſpirit-of-wine lamp, but alſo in a more convenient, more perfect, and much more certain manner than ever was effected in the dry way.

My whole apparatus for this purpoſe, and which may be carried in the pocket, is as follows:

1ſt, A ſmall cylindric furnace for placing the lamp in, four inches high and three in diameter, made of tin-plate, furniſhed with a door, and with draught-holes at the upper edge. The cover, which fits cloſely to it, has a round hole in the middle capable of admitting the crucible. This hole, to prevent the crucible from falling through, muſt have a ſmall rim bent inwards.

2d, A ſmall lamp capable of containing one ounce and a half of ſpirit of wine, furniſhed with a cover, a pair of ſmall tongs, and cotton to ſerve as wicks.

3d, A crucible capable of containing from two and a half to three ounces of fluid, together with a ſmall ſpatula for ſtirring round the matter, both of the fineſt ſilver.

The proceſs of reſolution is performed in the following manner:—Put the foſſil, reduced to a very fine powder, into the crucible, and mix it with the uſual quantity of cauſtic alkali diſſolved in diſtilled water; then light the lamp and boil the whole, taking care to ſtir it often round. As ſoon as the maſs is dry, pour hot [diſtilled] water over it; then boil it to dryneſs again; and repeat this proceſs, ſhould the refractory nature of the foſſil require it, for two or three times more, ſtill adding a like quantity of warm water.

If large tough bubbles ariſe during the proceſs of boiling, it is in general a ſign that the operation will be attended with a good reſult.

I found the experiment ſucceed in this manner with ſeveral of the hardeſt kind of foſſils; ſuch, for example, as the Siberian beryl, quartz, feld-ſpar, the topaz, and even the ſapphire: the laſt, however, was the moſt refractory; but it was equally ſo in the dry way, of which I have had an opportunity of being fully convinced.

Argillaceous earth, and alſo the metallic oxyds, for example thoſe of titanium, tin, iron, &c. (which, as is well known, loſe their property of being diſſolved in acids, either by too ſtrong drying, or by too high a degree of oxydation), by this eaſy treatment, in the wet way, with cauſtic alkali, may be rendered again ſoluble, in the courſe of five minutes, merely by once boiling them to dryneſs; an effect which, it was formerly believed, could not be produced but by bringing them to a ſtate of ignition with alkalies.

The chief advantages of this new method of reſolving foſſils in the wet way, are briefly as follow:

1ſt, The whole operation may be performed with the greateſt eaſe and convenience on a table in any apartment, even while reading a book.

2d, All the concomitating phenomena, and thoſe in particular which relate to the frequent change of colour, may be obſerved with the utmoſt accuracy.

3d, The operator has it perfectly in his power, when the foſſil operated upon is highly refractory, to promote the proceſs by the immediate addition of more cauſtic ley or water, without the leaſt interruption, but which cannot be done in the dry way without great loſs of time.

4th, By this method the operator, if he finds a new earth or metal, obviates entirely the ſuſpicion of its ariſing merely from ſome modification, or unmaſking of component parts already known, effected by the ſtrong heat.

5th, In the dry way, the operation often requires three, and even four hours; but by the propoſed wet way it may be effected, with much more certainty than before, in one hour and a half at moſt.

6th, The diſagreeable circumſtance, attended with many difficulties, of the ſolution of the foſſil ſubjected to the operation forming a kind of jelly with the acid, is entirely obviated by my method; at leaſt, as I have hitherto experienced in all my trials.

7th, The ſilver crucible beſides, being free from the danger of melting, as was formerly to be apprehended, ſuſtains very little damage by being uſed.

XI. *Curſory View of ſome of the late Diſcoveries in Science.*

[Continued from Page 152.]

VAUQUELIN, who has given ſo many excellent analyſes of minerals, has deſcribed the proceſſes he employed. The ſtone intended to be analyſed is firſt reduced to powder; it is then mixed with three times the quantity of potaſh, and, the whole being put into a crucible, is expoſed to a heat ſtrong enough to convert it into a frit. When the veſſel is taken from the fire, the frit is thrown into diſtilled water; and, when the matter is diſſolved, different chemical means are employed to diſcover the ſubſtances contained in the foſſil. The following are the characters by which each of the eight earths already known, and which may be contained in mineral ſubſtances, can be diſcovered:

1ſt, *Silex.* It diſſolves in the cauſtic alkalies, eſpecially by the help of heat, from which it is precipitated by acids; an exceſs of which rediſſolves it. The ſolution of this earth in acids forms itſelf into a jelly by evaporation, and when it has been dried it becomes inſoluble in theſe menſtrua: this furniſhes an excellent method for ſeparating it from other earths. In this ſtate it is white, granulated, dry to the touch, and perfectly inſoluble.

2d, *Alumine.* It diſſolves alſo in fixed alkalies and acids, from which it does not ſeparate itſelf, as ſilex does, by evaporation. It retains the water with force, and its parts become agglutinated and are brought into cloſer union by heat. In this ſtate it is white, ſemi-tranſparent, and adherent to the tongue. The combination of alumine with the ſulphuric acid gives, by the addition of a few drops of the ſulphat of potaſh, octaedral cryſtals of alum.

3d, *Zirconia.* It is not attacked by the cauſtic alkalies; acids, however, diſſolve it when it is very much divided, but not when it has been ſtrongly calcined; with the ſulphuric acid it forms an inſoluble ſalt; it adheres weakly to all the other acids, which it abandons at a very moderate degree of heat: in a word, when very much divided, it diſſolves in the alkaline carbonats, when completely ſaturated with carbonic acid. When it is pure, and ſtill contains water, it has a light ſtraw colour, is ſemi-tranſparent, and has a vitreous fracture like gum arabic; but when it has been calcined in a crucible, it is white, opake, rough to the touch, and diſſolves only with difficulty in acids.

4th, *Glucine.* This earth diſſolves, as alumine does, in acids and alkalies; but it diſſolves alſo in carbonat of ammonia, and furniſhes no alum with the ſulphuric acid or with potaſh. The ſalts which it forms with the acids are highly ſaccharine: when dry, it has a beautiful white colour, is exceedingly light, and ſoft to the touch, and has no ſavour. Its parts are not agglutinated by heat as thoſe of alumine are.

5th, *Magneſia.* It unites with all the acids, and forms with them ſalts exceedingly ſoluble and bitter: it is not precipitated from its ſolutions by carbonat of potaſh completely ſaturated with carbonic acid, and ammonia precipitates it only in part: it is not at all ſoluble in cauſtic alkalies, and it has a great affinity for alumine. When pure it is of a white colour, is exceedingly light, has no taſte, and is inſoluble in water.

6th, *Lime.* It combines with acids, and forms with them ſalts ſometimes ſoluble and ſometimes inſoluble: it does not diſſolve in alkalies: it diſſolves in water: its ſolution is rendered turbid by the carbonic acid, but not at all ſo by the ſulphuric acid. It is not precipitated from its ſolutions by ammonia, and it precipitates all the preceding earths. In its ſtate of purity it has an acrid cauſtic taſte, becomes hot with water, and a ſolution of it in that fluid does not cryſtalliſe.

7th, *Strontian.* It readily combines with acids, and with the ſulphuric acid forms a ſalt very little ſoluble: it diſſolves very abundantly in warm water, and its ſolution cryſtalliſes on cooling in very beautiful cryſtals, which form groupes almoſt like thoſe of ſal-ammoniac. The ſulphat of lime produces a precipitate in a ſolution of it: its combination with the muriatic acid diſſolved in alcohol burns with a purpliſh flame. This earth has an acrid taſte, and with water becomes ſtrongly heated.

8th, *Barytes.* This earth has many properties common with ſtrontian, from which it cannot eaſily be diſtinguiſhed but by its being more ſoluble in cold water and by its combination with the muriatic acid, being only a little ſoluble in alcohol, to the flame of which it does not communicate a purpliſh colour: it cryſtalliſes from its ſolution on cooling. It has an acrid taſte, becomes hot with water, forms an inſoluble ſalt with the ſulphuric acid, and decompoſes the alkaline ſulphats and carbonats as ſtrontian does, but with phenomena which to thoſe not much habituated to chemical labours differ by imperceptible ſhades, The ſame chemiſt has analyſed various ſpecies of the emerald. He obtained from the gray:

Silex	50·0
Alumine	07·0
Magneſia	08·0
Lime	17·0
Oxyd of iron	14·5

The green emerald gave:

Silex	51·0
Alumine	13·5
Magneſia	05·0
Lime	14·5
Oxyd of iron	08·0
Oxyd of copper	00·5
Oxyd of chromium	04·0

The green and whole emerald of Corſica gave:

Silex	50·0
Alumine	11·0
Magneſia	06·0
Lime	13·0
Oxyd of iron	05·5
Oxyd of copper	01·1
Oxyd of chrome	07·5

He concludes that this ſubſtance is coloured green by chrome.

He has analyſed the pyroxene of Ætna, and obtained from it:

Silex	52·00
Lime	13·20
Alumine	03·83
Magneſia	10·00
Oxyd of iron	14·66
Oxyd of manganeſe	02·00
Loſs	04·49

The granatite of Saint-Gothard, or the ſtaurolite, gave:

Alumine	47·06
Silex	30·59
Oxyd of iron	15·30

Lime - - -	03·00
Loſs - - -	04·50

The farinaceous chlorite gave:

Oxyd of iron - -	43·3
Silex - - -	26·0
Alumine - -	15·5
Magneſia - -	08·2
Muriat of potaſh -	02·0
Water - - -	04·0

The tourmaline of Ceylon gave:

Silex - - -	40·0
Alumine - - -	39·0
Lime - - -	04·0
Oxyd of mangaueſe -	02·5
Oxyd of iron - -	12·0
Loſs - - -	02·5

The zeolithe of Feroe gave:

Silex - - -	50·24
Alumine - -	29·30
Lime - - -	09·46
Water - - -	10·00
Loſs - - -	01·00

Lelievre has given an excellent deſcription of the lepidolite. It appears that it was firſt found by the abbé Poda, of Neuhaus, and that Born gave the firſt deſcription of it in the *Annales de Chimie* for 1791. That deſcribed by Lelievre is of a lilac colour, and ſeems to be compoſed of ſmall brilliant laminæ which might be taken for mica. Its ſpecific gravity, according to Haüy, is 2·8549. By the blow-pipe it is eaſily fuſed, and without ſwelling up. It gives a white ſemi-tranſparent glaſs filled with bliſters. An analyſis of it gave:

Silex - - -	54
Alumine - - -	20
Fluat of lime - -	04
Oxyd of manganeſe -	03
Oxyd of iron - -	01
Potaſh - - -	18

Vauquelin has analyſed the green feld-ſpar, called by ſome the amazon ſtone. It gave:

Silex - - -	62·83
Alumine - - -	17·00
Lime - - -	03·00
Oxyd of iron - -	01·00
Potaſh - - -	16·00
Loſs - - -	03·13

Here then we have potaſh extracted from the lepidolite and feld-ſpar, ſubſtances of the primary ſtrata.

Dr. Kennedy has extracted potaſh from pumice ſtone. The analyſis he gives of it is as follows:

Silex,
Alumine,
Oxyd of iron,
Potaſh.

The ſava of Ætna and baſaltes gave him ſoda or natron.

We have therefore ſeven mineral ſubſtances from which fixed alkalies have been extracted, *viz.*

1ſt, The aluminite of Tolfa, according to Monnet and Bergmann, contains potaſh.

2d, The ſiliceous depoſits of Geyer contain ſoda, according to Black.

3d, The leucite, according to Klaproth, contains potaſh.

4th, The lepidolite, according to Vauquelin and Klaproth, contains potaſh.

5th, Green feld-ſpar, according to Vauquelin, contains potaſh.

6th, Pumice ſtone contains potaſh, according to Kennedy.

7th, Lava and baſaltes contain ſoda, according to Kennedy *.

It has been propoſed, therefore, to make of the ſtones which contain alkalies, a genus under the name of the *alkaliferous*.

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* When we conſider how abundant muriat of ſoda is in nature, a ſubſtance which, with the aid of heat, forms a good flux for many ſubſtances, we need not be ſurpriſed at finding ſoda in any volcanic production.—Edit.

Napione has deſcribed a calcareous ſtone found in a vein of the primitive mountains of the valley of Seſſia. It is white, has a pearly ſplendour, and produces no ſenſible effervеſcence with acids. When calcined it makes good lime, when rubbed it becomes phoſphoreſcent. It gave when analyſed:

Lime	31·79
Magneſia	10·41
Oxyd of iron	01·00
Carbonic acid	42·00
Water	12·00

Poiret has made known a kind of foſſil wood, which he found near Laon. The inſide of this wood was covered with a ſtony ſubſtance exceedingly hard, ſiliceous, and depoſited in laminæ; but the external ſtrata, thoſe which appear to have belonged to the *liber*, and even to the *aubier*, appeared in long, capillary, brittle filaments, which, when ſtruck by the contact of the air, aſſumed the form of woolly flakes. They were interwoven in the ſame reticulated form as that which they affect in the living timber. In the laſt place, the part which conſtituted the *epidermis* had become a very fine black carbonaceous powder, which ſtained the fingers, and had all the characters of charcoal.

Pontier has ſent to the ſchool of mines at Paris, a new mineral ſubſtance found *à la Baſtide de la Carrade*, near Caſſin, in the department of the Var. It is of a dark brown colour almoſt like that of brown blend, and has metallic ſplendour: its ſpecific gravity is 4·0326. It is difficult to be fuſed by the blowpipe. The glaſs it produces is of a dirty greeniſh colour. Taſſaert has analyſed this ſubſtance, and found that it contains:

Chromic acid	63·6
Oxyd of iron	36·0
Loſs	00·4

But a new analyſis of it by Vauquelin and Taſſaert has given:

Chromic acid	43.0
Oxyd of iron	34·7
Alumine	20·3
Silex	02.0

Lelievre has deſcribed a mine of uranite diſcovered in the department of Saone and Loire.

The *perlſtein* of Werner (pearl-ſtone) is found in alternate ſtrata with argillaceous porphyry, reſting on trapp, on the road between Tokai and Kereſtour. This is what Fichtel calls *volcanic zeolite.* This ſubſtance is of a tarniſhed blue colour, has a reſinous fracture and a pearly appearance. The interior of it is formed by the union of ſeveral ſmall grains of black obſidian ſtone inclining to blue, each ſurrounded by a pearly pellicle of that ſubſtance. It is tranſlucid, fragile, and pretty light. Its gravity, according to Haüy, is 2·540.

Launoi has brought from Carboneira, in Spain, a ſubſtance analogous to the above, which, in imitation of the Germans, he calls *luchs-ſaphir.*

Werner is of opinion that this ſtone is not volcanic; he places it between the quartz and the hornſtein: the hornſtein is a kind of ſubſtance which the French, in imitation of the Swedes, call *petro-ſilex.*

Humboldt is of opinion that the pumice ſtone found on the peak of Teneriffe is only obſidian ſtone decompoſed by fire. According to this naturaliſt, its origin cannot be aſcribed to feld-ſpar. He collected a great many fragments, which were half obſidian and half fibrous pumice ſtone: they were of a black olive colour. He ſaw ſpecimens of the like kind in the cabinets of Madrid.

Fortis ſays that the Venetian ſhips bring from the iſland of Candia, as ballaſt, a kind of ſtones among which are obſerved large blocks of black glaſs, or obſidian, exceedingly pure, without any mixture of foreign bodies, which are viſibly changed into white pumice ſtone by a gradual tranſition, ſo that there are ſome fragments part of which is obſidian and part pumice ſtone.

The ſame thing is obſerved in the pure obſidian of Lipari, and the iſland of Procida near Naples.

Klaproth has analyſed a pumice ſtone, which gave:

Silex - - -	77·50
Alumine - - -	17·50
Oxyd of iron - -	01·75

Oxyd of mangaueſe -	0·1
Loſs - - -	3·25

We have, however, ſeen that Dr. Kennedy extracted from it potaſh.

Vauquelin has obtained potaſh alſo from feld-ſpar.

I have deſcribed in my Theory of the Earth[*] a ſtone of a roſe colour cryſtalliſed, almoſt hexaedral, with a triedral pyramid, which was ſold to me as coming from Dauria, and for that reaſon I gave it the name of *Daurite*. It has been examined alſo by Hermina, who calls it *Siberite*. He obſerved that it is pyro-electric, like the tourmalin. Garin and Pecheur analyſed it, and found that it contains:

Alumine - - -	48
Silex - - -	36
Lime - - -	03·5
Oxyd of manganeſe -	09

Abilgaard's *fluat of alumine*. This is a new ſtony ſubſtance found in Greenland. It is formed of whitiſh ſemi-tranſparent laminæ which ſubdivide themſelves into right priſms, which, according to Haüy, appear rectangular. Its ſpecific gravity, according to the ſame author, is 2,949. It fuſes by the flame of a taper, and flows like glaſs by the blowpipe: for this reaſon it has been called at Copenhagen *cryolite*. It is compoſed of alumine and fluoric acid.

Melarite. Klaproth has given this name to a ſtone hitherto called the black granite of Freſcati. The form of its cryſtals is a dodecaedron, with rhomboidal planes truncated on the twenty-four edges, which gives it thirty-ſix facets. Vauquelin has analyſed it.

Sage has publiſhed ſeveral memoirs.

1ſt, This chemiſt has confirmed, that what has been conſidered as a white volcanic glaſs is only a kind of chalcedony.

2d, He has obtained beautiful cryſtals of gold by reducing it by ether.

3d, He has analyſed the aſhes of vareeh, in which, he ſays, he found only a little ſoda; but an anonymous author informs us that he extracted from it a great deal; he thence concludes that thoſe ſent to Sage were not pure.

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[*] This article is taken from the *Journal de Phyſique*, of which Delametherie, who here ſpeaks, is the editor.—Edit.

4th, He has ſhown that antimony ſtrongly heated by the blowpipe inflames ſpontaneouſly.

5th, He has analyſed *pulvis ſtercoreus,* and extracted from it:

Mould - - - - - -	16·
Animal matter - - - - - -	16·
Vitriolic ſalt and calcareous marine ſalt -	2·
Calcareous earth - - - - -	36·
Divided quartz - - - - -	12·
Iron - - - - - - -	1·
Loſs - - - - - - -	17·

6th, The Dutch red precipitate or red oxyd of mercury gave him nearly a third of minium or red oxyd of lead.

7th, He conſiders calcareous earth as a combination of lime with *acidum pingue,* as Meyer had aſſerted before. The alkali, rendered cauſtic by lime, contains, according to him, a ſixth part of *acidum pingue.* A hundred grains of this acid, ſaturated with the phlogiſton of charcoal, produce 120 cubic inches of inflammable air. Scheele alſo extracted inflammable air from cauſtic alkali.

8th, He has examined a kind of argil near Mans, which he conſiders as a decompoſition of feld-ſpar.

9th, In examining what took place when the Odeon was burnt, he has ſhown, that in all conflagrations there is diſengaged a great deal of inflammable air which fills the building on fire. This air does not inflame as long as it is kept from coming into contact with the external air, but as ſoon as the latter penetrates to the interior of the building the inflammation becomes general; which makes people erroneouſly conclude that the building has caught fire in different places. He found on this occaſion a great deal of ſulphur, which he conſiders as a product of the decompoſition of the plaſter.

10th, He has examined the calcareous *ludi* of Dauphiny. In their fiſſures they contain beautiful cryſtals of quartz.

[To be continued.]

XII. *Deſcription of a Machine by which the Strength of Horſes may be employed to knead Paſte for baking Bread.*

THIS machine might be uſeful in armies and in the hoſpitals of large cities, as it would produce a great ſaving in regard to the number of hands employed. Two horſes, going round in a circle, give motion to a kind of peſtles, which, by their action, continually knead the paſte.

Fig. 1. (Plate VII.) repreſents a plan of the tub which contains the paſte. This tub, which is ſix feet in diameter in the inſide, conſiſts of thirty-eight ſtaves, and reſts on a wooden frame in the form of a croſs, the extremities of which are ſeen at A, B, C, D. (Fig. 1.) The bottom of the tub is formed of ſtrong planks, the ſtructure of which will be ſeen in the next figure.

Fig. 2. repreſents a tranſverſe ſection of the tub paſſing through the centre; E, the edge of the ſtaves thicker at the bottom than the top, and ſunk into the lower croſs, F. G, G, G, ſhows three iron hoops for binding together the ſtaves. The bottom, H, ſupports the wooden cylinder, I, which is in the centre of the tub, fixed in its place by three iron bolts ſimilar to that marked K, and ſeparated from each other as expreſſed by the points L, Fig. 3.

M, an iron arbor of the form of an inverted cone, the lower point of which reſts on the braſs bed, N, and is kept perpendicular by a braſs collar ſunk into the upper end of the wooden cylinder, having the form indicated in Fig. 3, with its three ears, made faſt by the ſcrews P, P, P.

This braſs collar is covered with a thin plate of iron marked Q, which receives the heads of the large bolts, K, which alſo paſs through a ſimilar plate, R, in the bottom of the tub, H: the bottom plate receives the nuts of the bolts.

S, Fig. 2, is the upper croſs, which turns, fixed to the iron arbor, M, and to which are ſuſpended the four wooden peſtles, two of which are viſible in this ſection at T and V, and of which the relative poſitions are indicated by X, X, X, X,

Fig. 4. Theſe peſtles or kneaders are let into the upper croſs, and kept faſt by means of horizontal bolts, Y, Y, fig. 2. Z, two arms, each twelve feet in length, to the extremity of which are ſuſpended the apparatus to which the harneſs for the horſes is attached.

Fig. 3. This figure of the cylinder in the centre of the tub, from which the parts were detached in the explanation of Fig. 2, is drawn on a ſcale double to that of the other figures to render it more diſtinct.

Fig. 4. Plan of the upper croſs above explained; where is ſeen, by the punctured circular ſegments, the path deſcribed by the kneaders in their revolution, the external angles of which are covered by each other about eight lines, in order that, ſucceeding each other in their circular progreſs, they may each take up a ſmall portion of the paſte which the preceding has kneaded.

NEW PUBLICATIONS.

The Geographical Syſtem of Herodotus examined and explained, &c. By James Rennell, *F. R. S. &c.* Nicol, Pall-Mall, 1800: 4to. 767 Pages.

BY the imperfection of aſtronomy, the comparative infrequency of the mutual intercourſe of nations, the want of the compaſs, the narrowneſs of the ſphere of knowledge and civility, and by various other cauſes, the antients were long hindered from making any conſiderable proficiency in geographical ſcience.

The writings of Herodotus, Strabo, Pauſanias, Xenophon, Cæſar, Arrian, Pliny, the Roman itineraries, and the great work of Ptolomy, contain the principal part of thoſe facts in antient geography which have been preſerved for our information.

They are, though imperfect, yet, for many reaſons, highly intereſting. The particulars of place, as of time, render the other facts of hiſtory more authentic and more capable of impreſſing the imagination. An acquaintance with the ſcenes of the moſt remarkable events of antient hiſtory, brings us to a knowledge of many circumſtances of thoſe events which we ſhould not have been otherwiſe able to diſcover. Facts the moſt important in the phyſical hiſtory of the globe are to be learned by a compariſon, which without ſome ſkill in antient geography would be impoſſible, of the antient with the modern appearance of all that is moſt ſtriking on its ſurface. Dates of time are not to be underſtood unleſs they be connected with thoſe of place, which it is the object of geography to aſcertain. Aſtronomy, natural hiſtory, natural philoſophy, geometry, navigation, and all the principal arts of civil life, are, in truth, intimately related to the ſcience of geography, either as ſupplying its facts and principles, or as deriving their beſt lights from it. A certain diſcovery of the ſcenes of ancient hiſtory may even ſeem, to a keen and perſpicacious mind, to annihilate the ages that have intervened, and to give us all the privileges of contemporaries of thoſe earlier races of men whoſe progreſs in civility we delight to contemplate. But for a knowledge of their geography, more than half the information which we find in the writings of the antients would be loſt to us.

Yet there is nothing more difficult in ſcience or erudition than to acquire a thorough knowledge of the facts of antient, in clear ſatisfactory compariſon with thoſe of modern, geography. The notions of the antients reſpecting the figure of the earth, and its relations to the heavenly bodies, were not, even after the time of Hipparchus, ſuch as to afford that accuracy in the diviſions of latitude and longitude without which local diſtances can never be very diſtinctly recorded. Their meaſures of diſtance were various and unfixed; and they truſted often, without actual meaſurement, to uncertain computation. The names which they apply to places vary or agree in different writings, with a very troubleſome irregularity. And the changes which have taken place on the face of the globe ſince our favourite antients wrote, have been ſo very conſiderable, that theſe, above every thing elſe, render all the parts of antient geography in the utmoſt degree difficult and obſcure.

We have accuſtomed ourſelves, therefore, to reſpect the labours of an Ortelius, a Cluverius, a Cellarius, a D'Anville, as highly beneficial alike to learning and to ſcience. We have been pleaſed with the reſearches of Saint Croix and of Vincent. We are grateful for the inſtruction which Mr. Rennell has already given concerning the antient geography of India. And we open, with eager curioſity and reſpect, this new volume, in which he attempts to illuſtrate the geography of the father of profane hiſtory.

This, it ſeems, is but a part of a great work on antient geography, upon which Mr. Rennell's leiſure has been for ſeveral years employed. We doubt not but the favourable reception of the preſent volume will encourage him to give the remainder of his labours on this plan, with as little delay as poſſible, to the public.

He begins with ſome prefatory matter on the general character of Herodotus as a geographer and hiſtorian: that Herodotus meant to produce in his work an univerſal hiſtory: that he viſited in perſon as wide a range as poſſible of the inhabited world, and, in regard to what he could not perſonally viſit, uſed the utmoſt diligence of inquiry: that his work faithfully repreſents the beſt truth of hiſtory which could in that age be known: that it was the ignorance alone of the Romans which led them to impeach his veracity: that modern inveſtigations have tended continually to vindicate, even in minute particulars, the truth of his narrative; are the chief facts which Mr. Rennell here ſtates.

The length of the Grecian ſtadium, and eſpecially of that of Herodotus, next engages the reſearches of Mr. Rennell. He aſcertains the length of the ſtadium employed by Herodotus in his ſtatements of diſtance, to have been the $\frac{1}{732}$ part of a geographical degree; that of Xenophon, $\frac{1}{750}$; that of Eratoſthenes, $\frac{1}{700}$; that of Strabo, the ſame; that of Pliny, $\frac{1}{727}$; that of Nearchus, $\frac{1}{720}$ and $\frac{1}{3}$; the mean ſtadium of the antients, $\frac{1}{718}$, or $505\frac{1}{2}$ Engliſh feet. The pace, in the meaſurements of the antients, he juſtly ſtates at five feet.

Herodotus's geography of Europe is next examined in the progreſs of this work. The Mediterranean Sea, and its European coaſts, were evidently well known to this hiſtorian. He knew a little, and but a little, of the weſtern ſhores of

Europe. With the extent and ſubdiviſions of the ſouthern countries on the Danube, he appears to have been well acquainted. His Scythia is the Weſtern Sarmatia of ſome later writers among the antients, the Ukraine of modern geography. But his notion of the form of this country was incorrect, as he ſuppoſed it to be one-half leſs than it really was. Mr. Rennell purſues his examination further, through the countries which Herodotus has deſcribed as bordering on Weſtern Scythia; remarks, after Gibbon, that the Genoeſe conſtructed thoſe fortifications of the Crimea, whoſe origin has been by ſome referred to earlier times; traces the courſe of the famous expedition of Darius into Weſtern Scythia; attends Herodotus into the countries beyond the Euxine ſea; deſcribes the Hyperboreans of the Romans as being the ſame with the Gog and Magog of the Scriptures, the Juje Majuje of the preſent Orientals; and takes occaſion to mention Herodotus, Pythagoras, Anacharſis, Peter Alexiowitz, and Sir Joſeph Banks, as the only men who have ſplendidly diſtinguiſhed themſelves by dangerous, ardent, and ſucceſsful travels in purſuit of knowledge.

The Aſia of Herodotus comes next under Mr. Rennell's examination. Its weſt and ſouth-weſt boundaries were the ſhores of the Arabian gulph, and of the Mediterranean and Euxine ſeas: its eaſtern limits were the country of the Oigurs, the deſert of Kobi, and India taken incluſively: on the ſouth, its only boundary known to Herodotus was the Erythrean ſea. The north of Aſia was to him unknown. He had little knowledge of India. And the length of Aſia eaſtward was leſs known to Herodotus than to ſome later geographers among the antients. With the bounds and extent of the Caſpian ſea he was better acquainted than the followers of Alexander. He knew it to be a lake; they ſuppoſed it to communicate with the great northern ocean. China, Chineſe and Weſtern Tartary, Thibet, the peninſula beyond the Ganges, and the greater part of Siberia, were entirely unknown to Herodotus. To him and the other Greeks of his time Eaſtern Scythia was little known. The Maſſagetæ, the Sacæ, and the Dahæ, were the great Scythian tribes. He has deſcribed the geography of the

Perſian empire as divided into twenty governments. Mr. Rennell examines, in minute detail, his account of each of theſe. The maritime ſtrength of the Phœnicians under the Perſian power is well marked by the fact which Mr. Rennell notices, that they furniſhed to the fleet of Xerxes 450 triremes, of which 150 were derived from their eſtabliſhment in the iſland of Cyprus. Of the Ganges it appears that Herodotus had never even heard. The public revenue of the Perſian empire was, according to his account, about $3\frac{1}{2}$ millions ſterling. The ſite of antient Babylon, as Mr. Rennell informs us upon good authority, is ſtill ſufficiently acceſſible, and is covered with ruins which it might be highly intereſting to examine.

Africa was beſt known to Herodotus where it is adjacent to the middle and the eaſtern limits of the Mediterranean ſea. Of its great rivers, he knew only the Niger and the Nile. He knew thoſe *oaſes*, or inſulated fertile ſpots in the ſandy deſert, which have been lately re-diſcovered by Brown. He knew that Africa had been circumnavigated. The voyage was performed at the rate of thirty-two geographical miles a day, and finiſhed in the ſpace of two years. The whole tract of the earth known to Herodotus might be meaſured by a radius of 1000 Britiſh miles, moving from the centre of Halicarnaſſus. The Seewa of Brown the traveller is the Oaſis of Ammon of Herodotus. Of the *lotos* there were two ſpecies; the one an aquatic plant, the other a dry ſhrub. The aquatic lotos was the ſame as the *colocaſſia*, and is ſtill abundant in Egypt. After finiſhing his diſcuſſion of the geography of Herodotus, Mr. Rennell enters into an intereſting illuſtration of the memoir of the voyage of Hanno. His work is accompanied with excellent maps, conſtructed upon thoſe principles and views which he explains in it. Under every leading head of his work, Mr. Rennell deſcends into much minuteneſs and accuracy of detail. It is interſperſed with various pleaſing digreſſions; and many curious articles of collateral information are incidentally thrown out in it.

Such are in general the plan, the execution, and the lead-facts of this work. It were eaſy to diſpute ſome of its poſitions; to wiſh, in one place, for more of ſcience; in another, for more of erudition; in a third, for greater accuracy of local knowledge. But it is written with ſuch patience and accuracy of inveſtigation, and in ſuch a ſpirit of candour; it elucidates ſo remarkably the pages of a favourite author; it connects Grecian learning ſo advantageouſly with the reſearches of modern travellers; and it makes ſo many intereſting additions to the facts of geographical ſcience, that we cannot doubt but the public muſt receive it as an important addition to this branch of literature: and we offer our beſt thanks to the author for having thus far executed ſo laborious and uſeful a taſk.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

ACADEMY OF BERLIN.

THE Royal Academy of Sciences at Berlin has propoſed the following prize queſtions for 1800, 1801, and 1802.

The Mathematical Claſs has continued for the year 1802, with a double prize, the following queſtion:

"Since, notwithſtanding the labours of the ableſt aſtronomers, there ſtill remain ſeveral points reſpecting the variation of the obliquity of the ecliptic to be cleared up, the Academy invites the learned to occupy themſelves again on this important object, and will adjudge a prize to that memoir which ſhall contain the moſt intereſting reſearches and the beſt illuſtrations reſpecting this important point."

The Claſs of Speculative Philoſophy propoſed the following queſtion for the year 1799:

"The important queſtion of the origin of our knowledge, agitated at all times, has been diſcuſſed at preſent with more intereſt than ever. It is certainly of great importance; and it is much to be wiſhed that the proofs on both ſides were carried to ſuch a degree of perfection, and rendered ſo ſatiſfactory, as to enable philoſophers to form a deciſive opinion on this point without falling into ſyncretiſm, which, by ſubſtituting indifference for intereſt, is highly detrimental to the progreſs of philoſophy. The Academy does not enter into the ideas of thoſe who conſider it as demonſtrated by mathematical evidence, that a part of our knowledge has its origin merely in the nature of our underſtanding: on the contrary, it is perſuaded that to this opinion eſſential objections may be made; objections which hitherto have not been anſwered in a ſatisfactory manner; eſpecially as it is convinced that there are very ſtrong proofs in favour of the opinion which deduces all our knowledge from experience, though theſe proofs, perhaps, have not yet been diſplayed in their full light. The Academy, being deſirous to contribute as much as poſſible to the ſolution of this problem, propoſes as the ſubject for which the Claſs of Philoſophy ought to adjudge a prize:

“ To demonſtrate, in an inconteſtable manner, the origin of all our knowledge; either by preſenting arguments never before employed, or arguments already employed, but exhibited with more clearneſs, and with ſuch force as to obviate every objection.”

But as none of the memoirs tranſmitted to the Academy have been thought ſatisfactory, the Academy finds itſelf obliged to defer the adjudication of the prize till the public ſitting of the month of Auguſt 1801. It repeats at the ſame time its invitation to philoſophers to contribute as much as in their power towards fixing the important point of the origin of human knowledge, by going back to the evidence, in order that the reſult may be, either that the ſcience of metaphyſics is a chimera, or that its principles are ſo determined that we cannot refuſe to them univerſality, and ſuch a comparative force of evidence, that there can no longer be room for diſputing reſpecting principles; and that all deviations muſt be conſidered merely as logical errors, of too little conſequence to promote a rage for ſyſtems and the ſpirit of party.

The Phyſical Claſs has propoſed for the year 1801 the following queſtion:

"Has electricity any action on matters which ferment? And in that caſe, What is its action? Is it favourable to fermentation, or does it prevent it? What advantages may be derived from an illuſtration of this point, in regard to improving the art of making wines, of brewing, and of diſtilling ſpirits?"

The learned of all countries, the ordinary members of the Academy excepted, are invited to exert their talents on theſe queſtions. The prizes are, a gold medal, equal in weight to fifty ducats, for the philoſophical queſtion; two medals for the mathematical, and a hundred crowns for the queſtion in phyſics. The papers, written in a legible hand, muſt be tranſmitted, with the uſual formalities, to the perpetual ſecretary of the Academy, before the firſt of May of the years above mentioned.

BATAVIAN SOCIETY AT HAARLEM.

On the 24th of May laſt this Society held their annual meeting, which the director opened by giving an account of the anſwers tranſmitted to the prize queſtions to be anſwered before the firſt of November 1799; the reſult of which was as follows:

On the queſtion reſpecting the utility and manner of employing ventilators in ſhips, two papers were tranſmitted, written in Dutch, one of which obtained a golden medal. The ſucceſsful candidate was L. Bicker, M. D. director of the Society of Experimental Philoſophy at Rotterdam.

On the other queſtions, reſpecting the improvement of downs, or ſandy and dry land, two papers were received without any devices. It was reſolved, therefore, that no further notice ſhould be taken of them; and that the queſtions ſhould not be repeated.

The three remaining queſtions to which no anſwers had been ſent, have been again propoſed in the following manner:

"I. What is the preſent ſtate of our knowledge in regard to the motion of the ſap in trees and plants? In what manner can a more complete knowledge be obtained of what is ſtill obſcure or dubious in this reſpect? and, Can conſequences uſeful for the cultivation of trees and plants be deduced from what has been already confirmed on this ſubject by deciſive experiments?

"II. As it is probable, if more attention were paid to thoſe phyſical cauſes which force back ſmoke into chimneys, that means might be deviſed to prevent apartments from being expoſed to it, except in the caſe when the chimneys are expoſed to violent winds, the Society requires:

"1ſt, A theory or brief phyſical explanation of the cauſes which force back ſmoke in chimneys, or prevent it from aſcending.

"2d, Rules, deduced from this theory, for building chimneys, and in which precepts muſt be given, taking into conſideration every poſſible circumſtance to prevent apartments from being expoſed to ſmoke.

"III. What indigenous plants, hitherto not uſed, are capable, according to well-atteſted experiments, of producing good dye ſtuffs, and might be employed with advantage for that purpoſe? and, What foreign plants might be reared in the leaſt fruitful and little cultivated parts of the republic, in order to procure dyes from them?"

The anſwers to theſe three queſtions muſt be tranſmitted to the Society before the firſt of November 1801.

For the preſent year, the Society has propoſed the following queſtions:

"I. What can we conſider as proved, or as highly probable, according to the lateſt aſtronomical obſervations, particularly thoſe of Herſchel and Schröter, in regard to the extent of the univerſe, and the order in which the heavenly bodies ſtand?"

In the anſwers to this queſtion, the Society requires that the authors will give a ſhort and comprehenſive view of the preſent ſtate of aſtronomy in regard to this point; and ſhow how improbable or ill founded ſome of the hypotheſes formed on this ſubject are.

"II. How far do we know, according to the lateſt improvements in the phyſiology of plants, in what manner the different kinds of earth promote the vegetation of plants?

and, What conſequences can be deduced from our knowledge on this ſubject in regard to the choice of manure, and the improvement of waſte and barren lands?

"III. Is the ſtudy of natural hiſtory of ſo much importance to youth that it deſerves to be conſidered as an eſſential part of a well regulated education? And if this be the caſe, what part of this ſcience deſerves the preference? and what is the propereſt method of exciting young perſons to ſtudy it, and of making it as uſeful to them as poſſible?

"IV. As it appears, from the experiments of Dr. Chladni, that, when a plate of metal or glaſs has been ſtrewed over with light ſand or duſt, if a violin bow be rubbed againſt it ſo as to produce a tone, the ſand aſſumes various figures; the Society requires a theory of this phenomenon, which ſhall be the reſult only of obſervation; and in particular:

"1ſt, As complete an account as poſſible of all the figures which each tone produces, and a claſſification of theſe figures according to their different kinds.

"2d, A phyſical explanation of the reaſon why the ſand aſſumes theſe figures, and of their relation to the reſpective tones."

The anſwers to the three firſt queſtions muſt be tranſmitted to the Society before the firſt of November 1801; that to the laſt, before the firſt of November 1802.

The following queſtions propoſed laſt year, to be anſwered before the firſt of November 1800, remain to be anſwered:

"I. What light has been thrown by the new chemiſtry on the phyſiology of the human body?

"II. How far has the light thrown by the new chemiſtry on the phyſiology of the human body contributed to make us better acquainted with the nature and cauſes of certain diſeaſes? and, What uſeful conſequences, more or leſs confirmed by experience, can be thence deduced for the practice of medicine?

"III. How far has the new chemiſtry ſerved to give us more accurate ideas of the efficacy of external or internal medicines long uſed or recently recommended? and of what advantage may a more accurate knowledge in this ſubject be in the treatment of certain diſeaſes?"

As moſt philoſophers, in applying the principles of the new chemiſtry to the phyſiology of the human body, to pathology, and therapeutics, have introduced hypotheſes built on too weak a foundation; and as this is, no doubt, prejudicial to the progreſs of theſe ſciences, to which the new chemiſtry promiſes ſo much improvement; if, according to the rule of Lavoiſier, we admit in chemiſtry, or in the application of its principle, thoſe things only which are ſupported by deciſive experiments; the Society wiſhes that thoſe inclined to anſwer theſe queſtions will accurately diſtinguiſh what is actually known, from what is merely hypothetical; and that, in regard to hypotheſes, they do confine themſelves merely to pointing them out, and to ſhowing, in a brief manner, the weakneſs of the grounds on which they reſt; as the principal object of theſe queſtions is to procure to the phyſicians and ſurgeons of the Batavian republic, who have not ſufficiently attended to the application of the principles of this ſcience, already well confirmed, to phyſiology, pathology, and therapeutics, appropriate treatiſes, from which they may eaſily be enabled to know what light the modern chemiſtry has really diffuſed on this branch of knowledge; and what is too ill founded, too haſtily adopted, or too doubtful to be depended on. The papers on theſe queſtions will be examined ſeparately. Thoſe inclined to anſwer more than one of them are requeſted to write diſtinct papers on each queſtion.

The following queſtions, firſt propoſed in 1794, and afterwards repeated in 1796, are alſo ſtill to be anſwered:

“ IV. A proper plan, founded on facts, and capable of being carried into execution, for improving and cultivating the waſte lands, whether ſands, moraſſes, or heaths, within the republic, and particularly in the diſtricts of Guelderland, Overyſſel, Drenthe, and Dutch Brabant.

“ V. What are the different breeds of ſheep in the United Provinces, and the different methods of breeding them? The Society wiſhes alſo to be informed of their ſize, their uſual weight, the meaſure and weight of their different limbs, the length and fineneſs of the wool, the number of lambs which they annually produce: whether the ſeveral joints, which Daubenton obſerved in ſome breeds, are to be conſidered as peculiar to one breed, or merely as a *luſus naturæ*; and whether, among all theſe breeds, there is one which originally belongs to Holland?"

The following queſtion, firſt propoſed in 1796, and repeated in 1799, is extended to the firſt of November 1802:

"A natural hiſtory and phyſical deſcription of the whale, which may ſerve to explain and point out the beſt means for diſcovering the places where theſe animals are to be found, as well as the eaſieſt and ſureſt means either known and uſed, or that can be employed, for immediately killing them, and of getting poſſeſſion of them in the ſpeedieſt and ſafeſt manner?"

The following queſtions, the firſt propoſed in 1790, the ſecond in 1794, the third and fourth in 1795, are ſtill continued for an unlimited period:

"I. What have the lateſt chemical diſcoveries taught concerning the nature of fermentation? and what advantage can be derived from this knowledge in regard to certain manufactures in which fermenting ſubſtances are employed?

"II. What have we been taught by experience in regard to the utility of certain animals, particularly in the Netherlands, which, according to appearance, are deſtructive? and what means muſt be employed to extirpate them?

"III. What indigenous plants, the virtues of which are as yet very little known, might be uſed with advantage in our *materia medica*, and be ſubſtituted for foreign plants?"

The papers which the Society expects on this queſtion, muſt contain proofs of the virtue and advantages of theſe indigenous plants, not merely from the teſtimony of foreigners, but from experiments and obſervations made originally in theſe provinces.

"IV. What indigenous plants, not yet uſed, might be employed as good and wholeſome nouriſhment? and what nutritive foreign plants might be cultivated in the United Provinces?"

The Society requeſts that the competitors will compreſs their papers as much as poſſible, and omit every thing that does not eſſentially relate to the queſtion. All the members of the Society are at liberty to be candidates, on condition that their papers are diſtinguiſhed by ſome device, and marked with the letter L.

The papers may be written either in Dutch, French, Latin, or German, and muſt be accompanied with a ſealed note containing the name and addreſs of the author. They muſt all be tranſmitted to Van Marum, ſecretary to the Society.

The prize for the beſt anſwer to any of the above queſtions is a gold medal, with the uſual impreſſion of the Society, containing on the edge the name of the author, and the year in which he obtained the prize; or, in lieu of it, at the option of the author, thirty ducats. Thoſe, however, who obtain the prize, or an acceſſit, ſhall not, without the expreſs permiſſion of the Society, publiſh their papers, either whole or in part, in the Tranſactions of the Society, nor in any other work, or ſeparately by themſelves.

The Society takes this opportunity of repeating, that they reſolved, in their ſitting of 1798, to examine, in every annual ſitting, whether, among the papers communicated in the courſe of the year, and not anſwers to queſtions propoſed, there are any reſpecting natural hiſtory or natural philoſophy which deſerve a particular reward; and, when this is the caſe, to adjudge to that paper, or, when there are ſeveral, to the moſt intereſting, a ſilver medal with the Society's impreſſion, and a preſent of ten ducats beſides.

ACADEMY OF LYONS.

C. Verniac, the prefect of the department of the Rhone, and C. Noel, commiſſioner-general of the police at Lyons, in conjunction with ſome members of the antient Academy of Lyons, have revived that valuable inſtitution under the title of the *Atheneum*. The number of the members of which it is to conſiſt has been fixed at forty-five; among whom are comprehended thoſe who made a part of the old academy, and who now are reduced to ſixteen. The Atheneum will have alſo fifteen aſſociates, and an unlimited number of correſpondents. It is divided into two claſſes;

one for the arts and ſciences, and the other for the *belles lettres* and fine arts. The firſt public ſitting was held on the 8th of Auguſt.

The Atheneum has already propoſed the following queſtions as the ſubjects of a prize:

"I. To point out thoſe indigenous mineral and vegetable ſubſtances which may be capable of producing a colouring principle applicable to ſilk, cotton, linen, hemp, woollen, and paper.

"II. To explain the proceſſes for extracting, fixing, and brightening the colours which may be furniſhed by ſimple indigenous ſubſtances not yet known in the art of dyeing."

The prize will be adjudged in the public ſitting of Meſſidor 24th, year 9. (July 14th, 1800.) It conſiſts of two medals of the value of 300 francs each. The ordinary members of the Atheneum are excluded from the competition.

MISCELLANEOUS ARTICLES.

HUMBOLDT'S TRAVELS THROUGH SPANISH AMERICA.

The following letter from this enterpriſing philoſopher to Fourcroy has lately been publiſhed in one of the French journals:

"Guyara, January 25, 1800.

"As the yellow fever, which deſolates this part of South America, obliges us to make our ſtay here as ſhort as poſſible, I ſnatch this opportunity of ſending you a few lines, and repeating, from the middle of the torrid zone, how much my thoughts are occupied with you and your illuſtrious colleagues, among whom I met with ſuch a flattering reception during my laſt ſtay at Paris. Since our departure from Saint Croix in Teneriffe, where I deſcended into the crater of the volcano, the atmoſpheric air being there at 0·8 of R. and at 0·19 of oxygen, I have written to you twice. I have ſent to Delambre and Lalande an extract of my aſtronomical labours; the longitude of ſome important places; an obſervation of the eclipſe on the 28th of October laſt; immerſions of the ſatellites, and reſearches reſpecting the intenſity of light of the auſtral ſtars, meaſured by means of diaphragms. I have addreſſed to the Inſtitute a chemical memoir on the phoſphoreſcence of the ſea; obſervations on a particular gas furniſhed by the fruit of the *coffea arabica* when expoſed to the ſun; on a ſnow white feld-ſpar, which, when moiſtened, abſorbs all the moiſture of the atmoſphere; experiments on the milk of the *cecropia peltata* and the *euphorbia curaſſavica*, which will form a ſupplement to your excellent memoir on the *cabout-chouc*, and to that of our friend Chaptal; and on the air which circulates in vegetables.

" The cruiſers which cover the ſeas here make me apprehend that a part of my letters may not have reached you; though I ſent them ſometimes by way of Guadaloupe, and ſometimes by that of Spain. Theſe few lines I have ſent by an American veſſel, which will ſail in a few days for Boſton; and, though they cannot reach you but through Hamburgh, they will, perhaps, be leſs liable to miſcarry. People here are accuſtomed to make four or five duplicates of their letters. But how can I find time, when I have ſo many things to obſerve, arrange, and calculate?

" I ſhall confine myſelf, therefore, to letting you once more know that I ſtill enjoy the beſt health poſſible, and that I am treated with the utmoſt kindneſs by the inhabitants of theſe countries: that the paſſports and letters of recommendation from the Spaniſh government procure me every facility for making reſearches uſeful to the ſciences; that none of my inſtruments, even the moſt delicate, ſuch as barometers, thermometers, hygrometers, Bordas' dipping needle, &c. are deranged: and that, at the extremity of the miſſionary eſtabliſhments among the Chayma Indians, in the mountains of Toumiriquiri, I have my laboratory mounted as if I were in the Hotel Boſton, in the Rue du Colombier. My fellow-traveller, Bonpland, educated in the *Jardin des Plantes*, becomes every day more valuable to me. To an extenſive knowledge of botany and comparative anatomy he unites indefatigable zeal. I hope one day to reſtore him to his country, worthy of attracting public attention. Never did any foreigner enjoy ſuch permiſſion as that granted to me by the king of Spain. This idea alone is capable of exciting us to redouble our activity. During the ſeven months we have been in this beautiful continent, we have dried (with duplicates) nearly 4000 plants, written more than 800 deſcriptions of new ſpecies or ſpecies little known, particularly new genera of palms, cryptogamia, bifaria, melaſtoma; collected inſects and ſhells, and made many drawings reſpecting the anatomy of marine worms; with obſervations on magnetiſm, electricity, humidity, the temperature of the atmoſphere, and the quantity of oxygen it contains. We have meaſured that immenſe and high chain of mountains extending to the coaſt of Paria, and examined their volcanoes, which vomit forth kindled inflammable air, ſulphur, and hydro-ſulphurous water. We have alſo collected many ſeeds, which we ſhall ſend off in three or four weeks for Europe, addreſſed to the *Jardin des Plantes*. We have ſpent five months in the interior of New Andaluſia and on the coaſts of Paria, where we experienced very violent earthquakes in the end of laſt year. One part of theſe countries is ſtill inhabited by the ſavage Indians, and others have begun to be cultivated only within five or ſix years. In what words ſhall I deſcribe to you the majeſty of the vegetation here; the foreſts of Ceiba, Hura, Hymenea, which the rays of the ſun never penetrate; the variety of the animals; the ſuperb plumage of the birds; the apes, the tigers; the hideous aſpect of the crocodiles (*caimans*) which ſwarm in the rivers, and of which ſome are thirty feet in length? From Cumana we proceeded to Caraccas, where we remained during the month of November and part of December: A charming capital ſituated in a valley which has 426 toiſes of elevation; and, though in latitude 10° 31′, enjoys the coolneſs, and I might ſay the cold, of Paris! From this place we aſcended to the ſummit of the famous Silla de Caraccas, or Sierra de Avila, where, at the height of 1316 toiſes, we diſcovered beautiful cryſtals of titanium. I found alſo dendrites ſimilar to thoſe of manganeſe, which are oxyd of titanium.

We ſhall proceed hence for Varina and the ſnow-covered mountains of Merida, the cataracts of Rio Nigro, and the unknown world of Oronoco, in order to return by Guyana to Cumana, from which we ſhall ſet out for the Havannah and Mexico.

“ We ſhall take care to tranſmit the ſeeds we have collected for the *Jardin des Plantes* at Paris, the Muſeum, and Sir Joſeph Banks, as we agreed with Juſſieu - - - -.

“ How much I lament the fate of Dolomieu, detained a priſoner in Sicily! If he ſhould return, communicate to him the following fact:—It is more than three years ſince I announced to Lametherie, that, in the primitive mountains of Italy, France, Swiſſerland, Poland, and I can now add Spain, there exiſts a *paralleliſm of direction* between the ſtrata of foliaceous granites, ſlate, micaceous ſchiſts, corneous ſchiſts: that theſe ſtrata incline (ſink down) towards the north-weſt, and that their direction makes with the axis of the globe an angle of 45° 57′: that this inclination and direction in no manner depend on the form and direction of the mountains: that it is not any way affected by the valleys, but that it announces a cauſe much greater, and more general: that it depends on a phenomenon of attraction which has acted at the time of the conſolidation of the globe. Having travelled over the greater part of Europe on foot, and with ſextants and compaſſes, I have a very extenſive collection of obſervations on that ſubject. My manuſcript on the identity of ſtrata in the conſtruction of the globe, is in the hands of my brother. I was employed on it ſince 1791, but it will not appear till I have ſeen more of the globe. To my great aſtoniſhment, I have obſerved in the Cordillera of Paria, New Andaluſia, New Barcelona, and Venzuela, that in the new world, near the equator, the ſtrata follow the ſame laws and the ſame paralleliſm. . . . .

“ You remember the laſt ingenious obſervations of Coulomb on the air which iſſues with exploſion from the trunks of trees when they are pierced. I have made experiments on the *cluſea roſea*, in which (in the interior of the *pneumato-chimifer* veſſels of Hedwig, the *vaſa cochleata* of Malpighi,) there circulates an immenſe quantity of air. This air contains as far as $\frac{35}{100}$ of oxygen. The leaves of the ſame tree, when expoſed to the ſun under water, do not give a cubic mellimetre of air. This air, which circulates, certainly ſerves, as in the animal body, to coagulate the fibrous part by abſorption of oxygen. The *cluſea* is a milky plant, and elaſtic gluten is formed in it.

“ Though the purity of the atmoſpheric air amounts here, particularly in the night, to 0·305 of oxygen, I have found that the air contained in the pods and capſules of equinoctial plants, for inſtance the *paullinia*, is more azotous than our atmoſpheric air. It does not exceed 0·24 or 0·25 of oxygen. The air in the *culmi geniculati* here, has only 0·15 of oxygen. All this proves that the air which circulates is purer, and that the air which is in a ſtate of reſt, depoſited in the capſules or *utriculi*, is leſs pure than atmoſpheric air. The former is recently produced by the organs which decompoſe the water: it proceeds to thoſe parts where it ought to ſerve, by its abundance of oxygen, to precipitate the fibrous principle to form the fibrous tiſſue. The other is the reſiduum of a gas which has already diſcharged theſe functions.

ALEX. HUMBOLDT.”

VACCINE INOCULATION.

Tranſlation of a Letter to George Pearſon, M. D. Phyſician to the Vaccine Inſtitution at London.

“ SIR, “ Paris, Germinal 14, (May 5,) 1800.

“ We have been informed by the public journals of the trials made in England with regard to the vaccine inoculation, the ſucceſs of which has, in a great meaſure, been owing to your *enlightened zeal*. Some friends of humanity, and of that ſcience which you cultivate in ſo diſtinguiſhed a manner, have formed a plan for introducing into France a practice which ſeems to promiſe advantages of ſo much magnitude. For this purpoſe they have propoſed a ſubſcription, which is now nearly filled up. You will find annexed a proſpectus of their plan; but, to carry into execution the views announced in it, we have need of your aſſiſtance; and we now ſolicit it with confidence.

“ Though ſome trials have been made here in regard to the vaccine inoculation, with which the School of Medicine has been more particularly occupied, we have not yet been able to obtain any poſitive reſult; the principal cauſe of which, no doubt, has been, the difficulty of procuring matter ſufficiently recent. Our veterinary doctors do not ſeem to be acquainted with this diſeaſe among cows. One inſtance only has occurred where they thought they obſerved this diſeaſe among theſe animals; and we embraced that opportunity of collecting the virus, which was employed, together with matter received from Geneva and England, but hitherto without any certain ſucceſs. We find ourſelves, therefore, deprived of the chief means neceſſary for commencing our trials; and we addreſs ourſelves to you to aſſiſt us in procuring them.

"We requeſt, but only in caſe you think theſe precautions neceſſary, to receive ſcabs or puſtules taken from cows or from perſons who have been inoculated, and threads or ſmall bits of ſponge, impregnated with matter taken from either; alſo, ſome of theſe ſcabs or puſtules quite freſh, and ſome of the matter produced in the place formed by the local affection of the part inoculated. The different kinds of matter or ſubſtances ought to be particularly marked. If your method of preſerving threads in azotic or hydrogen gas ſeems to be attended with any advantage, we expect from your zeal that you will employ it. In conjunction with our miniſter for foreign relations, we have take every neceſſary meaſure that the ſafeſt, the moſt convenient, and, in particular, the ſpeedieſt mode of conveyance ſhall be at your command. The Society, with equal eagerneſs, will make known the ſervices that you may render to it, and communicate to you the reſult of its reſearches, the merit of which will in a great meaſure belong to you, if, along with the matter which we requeſt, you will have the goodneſs to add ſome particular inſtructions, which we ſhall be very happy to follow.

"Health, eſteem, and devotion,

"*The Members of the Medical Committee of the Society formed at Paris for the Vaccine Inoculation*;

"PINEL, Profeſſor in the School of Medicine;

"THOURET, Director of the School of Medicine;

"Parfait, Inoculating Surgeon;
"Rouselle Chamseru, Phyſician to the Army;
"De la Porte, Phyſician to the Military Hoſpital;
"Huzard, Member of the Inſtitute;
"Tessier, Member of the Inſtitute;
"Cabannez, Member of the Inſtitute, Profeſſor in the School of Medicine."

The above letter, as appears, was addreſſed to Dr. Pearſon, who, however, in place of returning an anſwer in his own name, preſented it to the Vaccine Inſtitution, where it was determined that the commiſſion ſhould be executed by the medical eſtabliſhment. Accordingly, a packet containing vaccine matter on glaſs, on lancets, and on thread, incloſed in a bottle filled with hydrogen gas, and a letter of inſtructions ſigned by all the members of the medical eſtabliſhment, was ſent to Paris on the 12th of May laſt, with the permiſſion of Lord Grenville, through the hands of the French commiſſioner, Mr. Otto. How well the vaccine matter thus forwarded anſwered the ſanguine expectations of the Pariſian phyſicians, notwithſtanding the journey and the delay, is ſufficiently proved by the following extract from the *Gazette Nationale*; and other French papers furniſh ſimilar reports.

"*Gazette Nationale, Tridi,* 23 *Prairial, an.* 8 *de la Republique Françoiſe, une et indiviſible.*

"On the 13th of Prairial the vaccine inoculation was performed on thirty children with matter ſent from England, and according to the method recommended. Signs of infection manifeſted themſelves on nine of them at the periods and with the characters announced by Dr. Pearſon and other members of the committee inſtituted at London for the vaccine inoculation. It is to be remarked, that the Engliſh phyſicians expreſſed in their letter, 'that it ought to be eſteemed fortunate, on account of the time that muſt elapſe between collecting the matter and applying it, if, out of twenty individuals inoculated, one only ſhould take the diſeaſe.'

"On the 19th, 20th, and 21ſt, eighteen children, on whom the firſt inoculation produced no effect, were again inoculated with matter collected from thoſe on whom the firſt inoculation took effect. The committee continue their trials, and will inform the public of their further reſults.

"*In the name of the Medical Committee.* THOURET."

"The Vaccine Inſtitution, which has already been ſo uſeful in England, and which has communicated on the ſubject with the learned in various parts of Europe to introduce the cow-pock inoculation, has ſent us the following notice:

"Vaccine Inſtitution, Warwick-ſtreet,
Golden-ſquare, July 28, 1800.

"From the increaſe of patients, and the numerous applications for cow-pock matter, the ſubſcribers are hereby informed, that it has been reſolved to inoculate every Friday as well as Tueſday, under the direction of the medical eſtabliſhment, which at preſent is as follows:

"George Pearſon, M.D. F.R.S. L. Nihell, M.D. and Thomas Nelſon, M.D. phyſicians.

"Thomas Keate, Eſq. and John Ruſh, Eſq. conſulting ſurgeons.

"R. Keate, Eſq. John Gunning, Eſq. and J. Carpue, Eſq. ſurgeons.

"A. Brande, Eſq. F. Rivers, Eſq. and Mr. E. Brande, viſiting apothecaries.

"Mr. J. Lewis, reſident apothecary, to whom letters (poſt paid) may be addreſſed.

"The public are deſired to notice, that the medical eſtabliſhment do not authenticate vaccine matter unleſs it be delivered under the appropriate ſeal of the inſtitution.

"By order, WM. SANCHO, Secretary."

FRUCTIFICATION OF THE FUCUS NATANS.

A ſmall treatiſe has lately been publiſhed at Madrid by Hippolitus Ruiz, member of the Royal Medical Society of that city and botaniſt on the expedition to Peru, under the title of *De vera Fuci natantis fructificatione commentarius.* As this work is ſcarce, the following extract from it may not be unacceptable to our botanical readers:—The author, who is well known by his voyage to Peru for the improvement of botany, having found in the Atlantic, on his voyage back to Europe in the year 1788, a great quantity of floating ſea-graſs, *Fucus natans* Linn., he carefully examined the bladders of this marine production, which Linnæus erroneouſly conſidered as its fruit, but obſerved nothing in them that could induce him to accede to the opinion of that eminent naturaliſt. In the night of the firſt of Auguſt, when in the 24th degree of north latitude, the captain ſent him ſome of theſe fuci, which had been found adhering to one of the ropes, and which, as ſoon as agitated in the water, exhibited blueiſh luminous phoſphoric points. One of theſe he put into a veſſel filled with ſea-water, and obſerved that, when the plant was moved, a phoſphoric light ran along it, from the one end to the other, in a ſerpentine direction. In the day-time he found white threads adhering to it along its whole length, having at their extremities ſmall cups and ears, which he immediately conjectured to be the parts of fructification long ſought for without ſucceſs, but which were exceedingly different from thoſe which Linnæus and other botaniſts thought they obſerved in this ſpecies of plants. The fucus was kept four days in water; and always, when ſhaken in a dark place, it emitted a phoſphoric light which had a perfect reſemblance to that produced by mercury when agitated in vacuo. After the above period this phenomenon entirely ceaſed; but the author was able to exhibit it at pleaſure by obtaining freſh plants. The parts which produced the luminous appearance were the white threads, before mentioned, adhering to the *fucus*, which terminated either in ſmall cups or ears, or ears with ſmall knobs. He, however, did not find all theſe three forms of the ſuppoſed flowers on the ſame plant, but in different individuals. He found alſo that theſe bodies could not be eaſily ſeparated from the plant, even though the incruſtations with which the fucus was abundantly covered could be ſeparated with little difficulty. From this he concluded that the ſingle cups were the male flowers, the ears the hermaphrodite, and the ears with knobs the female.

A writer

A writer in one of the foreign journals, who mentions this work, makes the following obſervations on the ſubject:—“By the kindneſs of one of my friends I have obtained from Spain a ſpecimen of the ſuppoſed flowers of the *fucus natans*, and I muſt confeſs that the above deſcription of the parts does not appear to me ſatisfactory. Mr. Ruiz is certainly right when he ſays that the bladders in all the kinds of the *fuci* contain nothing that can ſerve for fructification; and the experiment he made by carefully ſtripping a *fucus natans* of all its bladders, in conſequence of which it ſunk in the water, clearly proves, what the German botaniſts have long ago obſerved, that the bladders of all the *fuci* ſerve to raiſe them like balloons, and to keep them floating in the water, as is the caſe with the *utricularia* found in our ditches. The real parts of fructification in this plant have been clearly pointed out in various kinds of it, by Roth and Stackhouſe. What the author conſiders as flowers are ſmall polypes, of that kind which Blumenbach calls *Brachionus*. Their white colour, the threads with which they were connected, and their phoſphoric light, (for he found nothing of the ſort in plants deſtitute of theſe appendages,) ought to have led him to the idea, that theſe bodies do not naturally belong to the plant, and are merely adventitious. Polypes have already often deceived botaniſts, who have conſidered them as the flowers of zoophytes without ſuſpecting that they might be the inhabitants of them.

“The account given by the author of the manner in which this plant grows is curious. The *fucus natans* has no roots, but excreſcences by which it adheres to the bottom of the ſea. As ſoon as it is full grown it becomes covered with bladders, which render it lighter than the water, and it then riſes to the ſurface. In this ſtate it floats on the waves from the month of July to September; but after this period it is no longer to be ſeen between the 24th and the 36th degree of north latitude. It ſometimes covers very extenſive parts of the ocean, which, by theſe means, have the appearance of meadows.”

COMBINATIONS OF LIGHT.

The celebrated Brugnatelli has lately publiſhed at Pavia, in the *Annali di Chimica*, the following obſervations on light:—Light is either chemically united with bodies, or merely accumulated and mixed in them in a mechanical manner but inviſible, or it is accumulated in bodies in a viſible ſtate.

Light chemically united with bodies, ſeparates itſelf from them in conſequence of its affinity for caloric. A red heat, however, is not neceſſary to diſengage light when in this ſtate. The black calx of manganeſe ſhines with great vivacity when thrown upon a very hot, but not ignited, plate of iron. The caſe is the ſame with the muriat of mercury (corroſive ſublimat), gray calx of mercury, gray calx of antimony, all calcareous ſalts, &c. Fine ſugar and ſugar of milk, when very dry and pounded, ſhine with great vivacity on a plate of iron which has been merely heated, but they do not exhibit the ſame phenomenon on iron brought to a red heat. Feathers, cotton, and wool, ſhine when drawn lightly over a piece of hot iron; and the ſpots of a playing card, under the mecircumſtances, emit a faint light. Camphor and choco thrown upon a piece of iron of this kind form luminous vapours. Several fluids, under the ſame circumſtances, produce the like effects. Oil of turpentine poured on hot iron gives a very perceptible light; and the caſe is the ſame with fat, oils, ſweat, wax, tallow, &c. Atmoſpheric air has not the leaſt influence in regard to the luminous appearance of theſe ſubſtances; as they ſhine alſo in vacuo, in carbonic acid gas, in hydrogen gas, &c. Nay, ſeveral bodies ſuffer their light to be diſengaged when they are immerſed in ſulphurous acid or in boiling oil. Different bodies, under ſuch circumſtances, give different quantities of light.

In regard to bodies in which light is merely accumulated mechanically, nothing is neceſſary to render it free but an approximation of their parts. In this manner the light is, as it were, ſqueezed out; as is the caſe with the quickſilver in the barometer when it becomes luminous, vitrioliſed tartar, and other ſalts, which ſhine at the moment when they cryſtalliſe. This phenomenon is exhibited alſo by the water of the ſea when it becomes phoſphoric, the eye when ſuddenly ſtruck, ſugar when pounded*, cream of tartar, alum and borax when ſtruck. Among mineral bodies, quartz in particular contains a great deal of accumulated light: to this head belongs the luminous appearance of certain plants.

Light is accumulated in a viſible ſtate in the ſo-called light magnets, which, when they have been expoſed for a conſiderable time to the light, imbibe a certain quantity of it, and then throw it out in the dark. Of theſe bodies, thoſe which deſerve the firſt place are the diamond, blende, and the carbuncle. *Lapis lazuli* throws out in the dark a great quantity of light which it imbibes in the day-time. The Bologna phoſphorus, which is nothing elſe than ſulphat of barytes, does not become luminous in the dark till it has been expoſed for ſome minutes to the light of the ſun; the caſe is the ſame with nitrat of lime. Brugnatelli ſaw a diamond become luminous in the dark after it had been expoſed to the light of a candle. Animal and vegetable matters, putrid fiſh, rotten wood, &c. ſhine in this manner; alſo individual parts of animals, the eyes of the hyæna and cats, the body of the glow-worm, &c.

MONSTROUS SERPENT.

The following article is copied from the Bombay Courier, of Auguſt 31ſt, 1799:

"A letter from Amboyna, received within theſe few days, gives the following account of an enormous ſnake, which made its appearance at a place called Golontala, in the iſland of Celebes. A Malay prow making for that port, and finding ſhe could not enter it before dark, came to anchor cloſe in ſhore for the night. One of the crew went on ſhore in queſt of betel-nut in the woods, and on his return lay down, as is ſuppoſed, to ſleep on the beach; a common cuſtom with people of that deſcription. In the courſe of the night his comrades in the boat heard his cries, and went immediately to his aſſiſtance, but too late, an immenſe ſnake having cruſhed him to death. Theſe people knowing that this kind of ſnake never diverts its attention from the prey which it has once ſeized until it is devoured, went boldly up to the monſter and cut its head off, carrying it and the body of the deceaſed on board their boat. The gentleman to whom we are indebted for this account, ſaw both the next morning; and found, on examining the latter, that the ſnake had ſeized the unhappy man by the right wriſt, where the marks of the animal's fangs were very diſtinct; and the mangled corpſe bore evident ſigns of having been cruſhed, by the ſnake twiſting itſelf round the head, neck, breaſt, and thigh. Our correſpondent extended the jaws of the ſnake, ſtiff as they were, wide enough to admit a body the ſize of a man's head; and the whole length of the animal is deſcribed to have been from about 28 to 30 feet, and equal in circumference to a moderate ſized man. By the account of the ſurvivors, this kind of ſnake ſwallows men and bullocks, after having cruſhed them, as in this inſtance; which our friend, judging from the capacity of the jaws in the ſtate in which he ſaw them, found no difficulty in believing: and this furniſhes a proof that ſimilar facts, ſtated by certain naturaliſts, to which many have refuſed their belief, are entitled to more deference than they generally meet with."

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* When a perſon champs fine ſugar between the teeth in the dark, the mouth appears to be full of fire

METEOROLOGY.

The heat of the preſent ſummer has been very great, and much more uniform and uninterrupted than for many preceding years. During the afternoon of ſeveral days this month, the thermometer in the neighbourhood of London, expoſed in a northerly aſpect, was as high as 86° of Fahr. In London it was higher, owing to the heat reflected from the ſurrounding buildings.

MEDICAL NOTICE.

Profeſſor Calliſen, of Copenhagen, read lately before the Royal Medical Society a paper in which he ſhowed that the external uſe of boiling water, in caſes of internal inflammation, produces a much ſpeedier and more efficacious effect than veſicatories. We have reaſon to hope that this matter will be further examined and illuſtrated.

DEATHS.

On the 17th of June, at Berlin, J. Abrahamſon, the celebrated dye-ſinker. He was born in 1722, in the duchy of Mecklenburgh-Strelitz, and learned, from a very indifferent artiſt of Liſſa, the art of engraving coats of arms, and of engraving on gems; but, being poſſeſſed of an excellent genius, he was able, by his own talents, combined with reflection, diligence, and cloſe application, to raiſe himſelf to that degree of celebrity to which he was ſo juſtly entitled. Without being able to draw or to model, he diſplayed in all his works great ability and readineſs. Among his principal productions may be reckoned his medals on the victories of Frederick II. during the ſeven years war, and particularly that on the battle of Torgau, after Ramler's idea and a drawing by Meils. He entered into the ſervice of his Pruſſian majeſty in 1750.

On the 20th of June, at Gottingen, that reſpectable veteran among the German mathematicians, Abraham Gotthelf Käſtner, in the eighty-firſt year of his age. He was a counſellor of ſtate to his Britannic majeſty, profeſſor of mathematics and natural philoſophy in the univerſity of Gottingen, member of the Royal Academy of Sciences of that city, of the Agricultural Society of Brunſwick Lunenburgh, of the Academies of Stockholm and Berlin, of the Electoral Academy of the Uſeful Sciences at Erfurt, of the Academy of Sciences at Bologna, &c. &c. Beſides poſſeſſing great mathematical knowledge, he was well acquainted with literature in general, and had a rich vein of wit, as appears by his proſe writings and his epigrams.

THE

PHILOSOPHICAL MAGAZINE,

SEPTEMBER 1800.

I. *On the Electricity excited by the mere Contact of conducting Subſtances of different Kinds. In a Letter from Mr.* ALEXANDER VOLTA, *F.R.S. Profeſſor of Natural Philoſophy in the Univerſity of Pavia, to the Right Hon. Sir* JOSEPH BANKS, *Bart. K.B. P.R.S.**

Como in the Milaneſe, March 20, 1800.

AFTER a long ſilence, for which I ſhall offer no apology, I have the pleaſure of communicating to you, and through you to the Royal Society, ſome ſtriking reſults I have obtained in purſuing my experiments on electricity excited by the mere mutual contact of different kinds of metal, and even by that of other conductors, alſo different from each other, either liquid or containing ſome liquid, to which they are properly indebted for their conducting power. The principal of theſe reſults, which comprehends nearly all the reſt, is the conſtruction of an apparatus having a reſemblance in its effects (that is to ſay, in the ſhock it is capable of making the arms, &c. experience) to the Leyden flaſk, or, rather, to an electric battery weakly charged acting inceſſantly, which ſhould charge itſelf after each exploſion; and, in a word, which ſhould have an inexhauſtible charge, a perpetual action or impulſe on the electric fluid; but which differs from it eſſentially both by this continual action, which is peculiar to it, and becauſe, inſtead of conſiſting, like the common electric jars and batteries, of one or more inſulating plates or thin ſtrata of thoſe bodies which are alone thought to be *electric*, armed with conductors, or bodies called *non-electric*, this new apparatus is formed merely of ſeveral of the latter bodies, choſen from among thoſe which are the beſt conductors, and therefore the moſt remote, as has hitherto been believed, from the electric nature. The apparatus to which I allude, and which will, no doubt, aſtoniſh you, is only the aſſemblage of a number of good conductors of different kinds arranged in a certain manner. Thirty, forty, ſixty, or more pieces of copper, or rather ſilver, applied each to a piece of tin, or zinc, which is much better, and as many ſtrata of water, or any other liquid which may be a better conductor, ſuch as ſalt water, ley, &c. or pieces of paſteboard, ſkin, &c. well ſoaked in theſe liquids; ſuch ſtrata interpoſed between every pair or combination of two different metals in an alternate ſeries, and always in the ſame order of theſe three kinds of conductors, are all that is neceſſary for conſtituting my new inſtrument, which, as I have ſaid, imitates the effects of the Leyden flaſk, or of electric batteries, by communicating the ſame ſhock as theſe do; but which, indeed, is far inferior to the activity of theſe batteries when highly charged, either in regard to the force and noiſe of the exploſions, the ſpark, the diſtance at which the diſcharge may be effected, &c. as it equals only the effects of a battery very weakly charged, though of immenſe capacity: in other reſpects, however, it far ſurpaſſes the virtue and power of theſe batteries, as it has no need, like theſe, of being previouſly charged by means of foreign electricity, and as it is capable of giving a ſhock every time it is properly touched, however often it may be.

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* Tranſlated from the author's paper publiſhed in French in the Philoſophical Tranſactions for 1800, part 2.

To this apparatus, much more ſimilar at bottom, as I ſhall ſhow, and even ſuch as I have conſtructed it, in its form to the *natural electric organ* of the torpedo or electric eel, &c. than to the Leyden flaſk and electric batteries, I would wiſh to give the name of the *artificial electric organ*: and, indeed, is it not, like it, compoſed entirely of conducting bodies? Is it not alſo active of itſelf without any previous charge, without the aid of any electricity excited by any of the means hitherto known? Does it not act inceſſantly, and without intermiſſion? And, in the laſt place, is it not capable of giving every moment ſhocks of greater or leſs ſtrength, according to circumſtances—ſhocks which are renewed by each new touch, and which, when thus repeated or continued for a certain time, produce the ſame torpor in the limbs as is occaſioned by the torpedo, &c.?

I ſhall now give a more particular deſcription of this apparatus and of others analogous to it, as well as of the moſt remarkable experiments made with them.

I provide a few dozens of ſmall round plates or diſks of copper, braſs, or rather ſilver, an inch in diameter more or leſs (pieces of coin for example), and an equal number of plates of tin, or, what is better, of zinc, nearly of the ſame ſize and figure. I make uſe of the term *nearly*, becauſe great preciſion is not neceſſary, and the ſize in general, as well as the figure of the metallic pieces, is merely arbitrary: care only muſt be taken that they may be capable of being conveniently arranged one above the other, in the form of a column. I prepare alſo a pretty large number of circular pieces of paſteboard, or any other ſpongy matter capable of imbibing and retaining a great deal of water or moiſture, with which they muſt be well impregnated in order to enſure ſucceſs to the experiments. Theſe circular pieces of paſteboard, which I ſhall call moiſtened diſks, I make a little ſmaller than the plates of metal, in order that, when interpoſed between them, as I ſhall hereafter deſcribe, they may not project beyond them.

Having all theſe pieces ready in a good ſtate, that is to ſay, the metallic diſks very clean and dry, and the non-metallic ones well moiſtened with common water, or, what is much better, ſalt water, and ſlightly wiped that the moiſture may not drop off, I have nothing to do but to arrange them, a matter exceedingly ſimple and eaſy.

I place then horizontally, on a table or any other ſtand, one of the metallic pieces, for example one of ſilver, and over the firſt I adapt one of zinc; on the ſecond I place one of the moiſtened diſks, then another plate of ſilver followed immediately by another of zinc, over which I place another of the moiſtened diſks. In this manner I continue coupling a plate of ſilver with one of zinc, and always in the ſame order, that is to ſay, the ſilver below and the zinc above it, or *vice verſa*, according as I have begun, and interpoſe between each of theſe couples a moiſtened diſk. I continue to form, of ſeveral of theſe ſtories, a column as high as poſſible without any danger of its falling.

But, if it contain about twenty of theſe ſtories or couples of metal, it will be capable not only of emitting ſigns of electricity by Cavallo's electrometer, aſſiſted by a condenſer, beyond ten or fifteen degrees, and of charging this condenſer by mere contact ſo as to make it emit a ſpark, &c. but of giving to the fingers with which its extremities (the bottom and top of the column) have been touched ſeveral ſmall ſhocks, more or leſs frequent, according as the touching has been repeated. Each of theſe ſhocks has a perfect reſemblance to that ſlight ſhock experienced from a Leyden flaſk weakly charged, or a battery ſtill more weakly charged, or a torpedo in an exceedingly languiſhing ſtate, which imitates ſtill better the effects of my apparatus by the ſeries of repeated ſhocks which it can continually communicate.

To obtain ſuch ſlight ſhocks from this apparatus which I have deſcribed, and which is ſtill too ſmall for great effects, it is neceſſary that the fingers, with which the two extremities are to be touched at the ſame time, ſhould be dipped in water, ſo that the ſkin, which otherwiſe is not a good conductor, may be well moiſtened. To ſucceed with more certainty, and receive ſtronger ſhocks, a communication muſt be made, by means of a metallic plate ſufficiently large, or a large metallic wire, between the bottom of the column (that is to ſay, the lower piece of metal,) and water contained in a baſon or large cup, in which one, two, or three fingers, or the whole hand is to be immerſed, while you touch the top or upper extremity (the uppermoſt or one of the uppermoſt plates of the column) with the clean extremity of another metallic plate held in the other hand, which muſt be very moiſt, and embrace a large ſurface of the plate held very faſt. By proceeding in this manner, I can obtain a ſmall pricking or ſlight ſhock in one or two articulations of a finger a finger immerſed in the water of the baſon, by touching, with the plate graſped in the other hand, the fourth or even third pair of metallic pieces. By touching then the fifth, the ſixth, and the reſt in ſucceſſion till I come to the laſt, which forms the head of the column, it is curious to obſerve how the ſhocks gradually increaſe in force. But this force is ſuch, that I receive from a column formed of twenty pairs of pieces (not more) ſhocks which affect the whole finger with conſiderable pain if it be immerſed alone in the water of the baſon; which extend (without pain) as far as the wriſt, and even to the elbow, if the whole hand, or the greater part of it, be immerſed; and are felt alſo in the wriſt of the other hand.

I ſtill ſuppoſe that all the neceſſary attention has been employed in the conſtruction of the column, and that each pair or couple of metallic pieces, reſulting from a plate of ſilver applied over one of zinc, is in communication with the following couple by a ſufficient ſtratum of moiſture, conſiſting of ſalt water rather than common water, or by a piece of paſteboard, ſkin, or any thing of the ſame kind well impregnated with this ſalt water. The diſk muſt not be too ſmall, and its ſurface muſt adhere cloſely to thoſe of the metallic plates between which it is placed. This exact and extenſive application of moiſtened diſks is very important, whereas the metallic plates of each pair may only touch each other in a few points, provided that their contact is immediate.

All this ſhows that, if the contact of the metals with each other in ſome points only be ſufficient (as they are excellent conductors) to give a free paſſage to a moderately ſtrong current of electricity, the caſe is not the ſame with liquids, or bodies impregnated with moiſture, which are conductors much leſs perfect; and which, conſequently, have need of more ample contact with metallic conductors, and ſtill more with each other, in order that the electric fluid may eaſily paſs, and that it may not be too much retarded in its courſe; eſpecially when it is moved with very little force, as in the preſent caſe.

In a word, the effects of my apparatus, that is to ſay, the ſhocks felt, are conſiderably more ſenſible in proportion as the temperature of the ambient air, or that of the water or moiſtened diſks which enter into the compoſition of the column, and that of the water even in the baſon, is warmer, as heat renders the water a better conductor. But almoſt all the ſalts, and particularly common ſalt, will render it a ſtill better. This is one of the reaſons, if not the only one, why it is ſo advantageous that the water of the baſon, and, above all, that interpoſed between each pair of metallic plates, as well as the water with which the circular pieces of paſteboard are impregnated, &c. ſhould be ſalt water, as already obſerved.

But all theſe means and all theſe attentions have only a limited advantage, and will never occaſion your receiving very ſtrong ſhocks as long as the apparatus conſiſts but of one column, formed only of twenty pair of plates, even though they may conſiſt of the two metals propereſt for theſe experiments, *viz.* ſilver and zinc; for if they were ſilver and lead, or tin, or copper and tin, the half of the effect would not be produced, unleſs the weaker effect of each pair were ſupplied by a much greater number. What really increaſes the electric power of this apparatus, and to ſuch a degree as to make it equal or ſurpaſs that of the torpedo or electric eel, is the number of plates arranged in ſuch a manner, and with the attention before mentioned. If to the twenty pairs above deſcribed twenty or thirty others be added diſpoſed in the ſame order, the ſhocks which may be communicated by a column lengthened in this manner will be much ſtronger, and extend to both arms as far as the ſhoulder; and eſpecially of that, the hand of which has been immerſed in the water: this hand, with the whole arm, will remain more or leſs benumbed, if by frequently renewing the touches theſe ſhocks be made to ſucceed each other rapidly, and without intermiſſion. This will be the caſe if the whole hand, or the greater part of it, be immerſed in the water of the baſon; but if only one finger be immerſed, either wholly or in part, the ſhocks being almoſt entirely concentrated in it alone, will become ſo much the more painful, and ſo acute as to be ſcarcely ſupportable.

It may readily be conceived that this column, formed of forty or fifty couples of metals, which gives ſhocks more than moderate to both the arms of one perſon, is capable of giving ſenſible ſhocks alſo to ſeveral perſons, holding each other by the hands (ſufficiently moiſt) ſo as to form an uninterrupted chain.

I ſhall now return to the mechanical conſtruction of my apparatus, which is ſuſceptible of ſeveral variations, and deſcribe not all thoſe which I have invented or made, either on a ſmall or a large ſcale, but only a few, which are either curious or uſeful, which exhibit ſome real advantage, as being eaſier or ſooner conſtructed, and which are certain in their effects, or can be longer preſerved in good order.

I ſhall begin by one which, uniting nearly all theſe advantages, differs moſt in its figure from the columnar apparatus above deſcribed, but which is attended with the inconvenience of being much more voluminous. This new apparatus, which I ſhall call a *couronne de taſſes* (a chain of cups), is repreſented Plate VIII. fig. 1.

I diſpoſe, therefore, a row of ſeveral baſons or cups of any matter whatever, except metal, ſuch as wood, ſhell, earth, or rather glaſs (ſmall tumblers or drinking glaſſes are the moſt convenient), half filled with pure water, or rather ſalt water or ley: they are made all to communicate by forming them into a ſort of chain, by means of ſo many metallic arcs, one arm of which, S*a*, or only the extremity S, immerſed in one of the tumblers, is of copper or braſs, or rather of copper plated with ſilver; and the other, Z*a*, immerſed into the next tumbler, is of tin, or rather of zinc. I ſhall here obſerve, that ley and other alkaline liquors are preferable when one of the metals to be immerſed is tin: ſalt water is preferable when it is zinc. The two metals of which each arc is compoſed, are ſoldered together in any part above that which is immerſed in the liquor, and which muſt touch it with a ſurface ſufficiently large: it is neceſſary therefore that this part ſhould be a plate of an inch ſquare, or very little leſs; the reſt of the arc may be as much narrower as you chooſe, and even a ſimple metallic wire. It may alſo conſiſt of a third metal different from the two immerſed into the tumblers, ſince the action on the electric fluid which reſults from all the contacts of ſeveral metals that immediately ſucceed each other, or the force with which this fluid is at laſt impelled, is abſolutely the ſame, or nearly ſo, as that which it would have received by the immediate contact of the firſt metal with the laſt without any intermediate metals, as I have aſcertained by direct experiments, of which I ſhall have occaſion to ſpeak hereafter.

A ſeries of 30, 40, or 60 of theſe tumblers connected with each other in this manner, and ranged either in a ſtraight or curved line, or bent in every manner poſſible, forms the whole of this new apparatus, which at bottom and in ſubſtance is the ſame as the other columnar one above deſcribed; as the eſſential part, which conſiſts in the immediate communication of the different metals which form each couple, and the mediate communication of one couple with the other, *viz.* by the intervention of a humid conductor, exiſt in the one as well as the other.

In regard to the manner of trying theſe tumblers, and the different experiments for which they may be employed, there is no need of ſaying a great deal after the ample explanation I have already given reſpecting the columnar apparatus. It may be readily comprehended, that to obtain a ſhock it will be ſufficient to immerſe one hand into one of the tumblers, and a finger of the other hand into another of the tumblers at a conſiderable diſtance from the former: that this ſhock will be ſtronger the further theſe glaſſes are from each other; that is to ſay, in proportion to the number of the intermediate glaſſes, and conſequently, that the ſtrongeſt ſhock will be received when you touch the firſt and laſt end of the chain. It will be readily comprehended alſo, how and why the experiments will ſucceed much better by graſping and holding faſt in one hand, well moiſtened, a pretty large plate of metal (in order that the communication may be more perfect, and formed in a great number of points), and touching with this plate the water in the tumbler, or rather the metallic arc, while the other is immerſed in the other diſtant tumbler, or touches with a plate, graſped in the like manner, the arc of the latter. In a word, one may comprehend and even foreſee the ſucceſs of a great variety of experiments which may be made with this apparatus or chain of cups much more eaſily, and in a manner more evident, and which, if I may be allowed the expreſſion, ſpeak more to the eyes than thoſe with the columnar apparatus. I ſhall therefore forbear from deſcribing a great number of theſe experiments, which may be eaſily gueſſed; and ſhall relate only a few which are no leſs inſtructive than amuſing.

Let three twenties of theſe tumblers be ranged, and connected with each other by metallic arcs, but in ſuch a manner, that, for the firſt twenty, theſe arcs ſhall be turned in the ſame direction; for example, the arm of ſilver turned to the left, and the arm of zinc to the right; and for the ſecond twenty in a contrary direction, that is to ſay, the zinc to the left, and the ſilver to the right: in the laſt place, for the third twenty, the ſilver to the left, as is the caſe in regard to the firſt. When every thing is thus arranged, immerſe one finger in the water of the firſt tumbler, and, with the plate graſped in the other hand, as above directed, touch the firſt metallic arc (that which joins the firſt tumbler to the ſecond), then the other arc which joins the ſecond and third tumbler, and ſo on, in ſucceſſion, till you have touched them all. If the water be very ſalt and luke-warm, and the ſkin of the hands well moiſtened and ſoftened, you will already begin to feel a ſlight ſhock in the finger when you have touched the fourth or fifth arc (I have experienced it ſometimes very diſtinctly by touching the third), and by ſucceſſively proceeding to the ſixth and the ſeventh, &c. the ſhocks will gradually increaſe in force to the twentieth arc, that is to ſay, to the laſt of thoſe turned in the ſame direction; but by proceeding onwards to the 21ſt, 22d, 23d, or 1ſt, 2d, 3d, of the ſecond twenty, in which they are all turned in a contrary direction, the ſhocks will each time become weaker, ſo that at the 36th or 37th, they will be imperceptible, and be entirely null at the 40th, beyond which (and beginning the third twenty, oppoſed to the ſecond and analogous to the firſt,) the ſhocks will be imperceptible to the 44th or 45th arc; but they will begin to become ſenſible, and to increaſe gradually, in proportion as you advance to the 60th, where they will have attained the ſame force as that of the 20th arc.

If the twenty arcs in the middle were all turned in the ſame direction as the preceding twenty and the following twenty, that is to ſay, if the whole 60 conſpired to impel the electric fluid in the ſame direction, it may readily be comprehended how much greater the effect will be at the end, and how much ſtronger the ſhock; and it may be comprehended, in general, to what point it muſt be weakened in all caſes where a greater or ſmaller number of theſe forces act contrary to each other by an inverted poſition of metals.

If the chain be in any part interrupted, either by one of the tumblers being empty of water, or one of the metallic arcs being removed or divided into two pieces, you will receive no ſhock when you immerſe your finger into the water of the firſt and another into that of the laſt veſſel; but you will have it ſtrong or weak, according to circumſtances (leaving theſe fingers immerſed), at the moment when the interrupted communication is reſtored; at the moment when another perſon ſhall immerſe into the two tumblers, where the arc is wanting, two of his fingers (which will alſo receive a ſlight ſhock), or rather, when he ſhall immerſe the ſame arc which has been taken away, or any other; and in the caſe of the arc ſeparated into two pieces, at the moment when theſe pieces are again brought into mutual contact (in which caſe the ſhock will be ſtronger than in any other); and, laſtly, in the caſe of the empty tumbler, at the moment when water poured into it ſhall riſe to the two metallic arms immerſed in this cup which before were dry.

When the chain of cups is of ſufficient length, and capable of giving a ſtrong ſhock, you will experience one, though much weaker, even though you keep immerſed two fingers, or the two hands, in one baſon of water of pretty large ſize, in which the firſt and laſt metallic arcs are made to terminate: provided that either of theſe hands thus immerſed, or rather both of them, be kept reſpectively in contact, or nearly in contact, with theſe arcs, you will, I ſay, experience a ſhock at the moment when (the chain being interrupted in any part) the communication is reſtored, and the circle completed in any of the ways before mentioned. One might be ſurpriſed that in this circle the electric current having a free paſſage through an uninterrupted maſs of water, that which fills the baſon, ſhould quit this good conductor to throw itſelf and purſue its courſe through the body of the perſon who holds his hands immerſed in the ſame water, and thus to take a longer paſſage. But the ſurpriſe will ceaſe if we reflect, that living and warm animal ſubſtances, and above all, their humours, are, in general, better conductors than water. As the body, then, of the perſon who immerſes his hands in the water, affords an eaſier paſſage than this water does to the electric current, the latter muſt prefer it though a little longer. In a word, the electric fluid, when it muſt traverſe imperfect conductors in a large quantity, and particularly moiſt conductors, has a propenſity to extend itſelf in a larger ſtream, or to divide itſelf into ſeveral, and even to purſue a winding courſe, as it thereby finds leſs reſiſtance than by following one ſingle channel, though ſhorter; in the preſent caſe it is only a part of the electric current, which, leaving the water, purſues this new route through the body of the perſon, and traverſes it from the one arm to the other: a greater or leſs part paſſes through the water in the veſſel. This is the reaſon why the ſhock experienced is much weaker than when the electric current is not divided when the perſon alone forms the communication between one arc and another, &c.

From theſe experiments one might believe, that when the torpedo wiſhes to communicate a ſhock to the arms of a man or to animals which touch it, or which approach its body under the water (which ſhock is much weaker than what the fiſh can give out of the water), it has nothing to do but to bring together ſome of the parts of its electric organ in that place, where, by ſome interval, the communication is interrupted, to remove the interruptions from between the columns of which the ſaid organ is formed, or from between its membranes in the form of thin diſks, which lie one above the other from the bottom to the ſummit of each column: it has, I ſay, nothing to do but to remove theſe interruptions in one or more places, and to produce there the requiſite contact, either by compreſſing theſe columns, or by making ſome moiſture to flow in between the pellicles or diaphragms which have been ſeparated, &c. This is what may be, and what I really conclude to be, the taſk of the torpedo when it gives a ſhock; for all the reſt, the impulſe and movement communicated to the electric fluid, is only a neceſſary effect of its ſingular organ, formed, as is ſeen, of a very numerous ſeries of conductors, which I have every reaſon to believe ſufficiently different from each other to be *exciters* of the electric fluid by their mutual contacts; and to ſuppoſe them ranged in a manner proper for impelling that fluid with a ſufficient force from top to bottom, or from the bottom to the top, and for determining a current capable of producing the ſhock, &c. as ſoon and as often as all the neceſſary contacts and communications take place.

But let us now leave the torpedo, and its *natural electric organ*, and return to the *artificial electric organ* of my invention, and particularly to my firſt *columnar apparatus*, that which imitates the firſt even in its form (for that compoſed of tumblers is different in that reſpect). I might ſay ſomething alſo in regard to the conſtruction of the ſaid apparatus with tumblers or *a chain of glaſſes*; for example, that the firſt and laſt tumbler ſhould be of ſuch a ſize that, when neceſſary, the whole hand might be immerſed in it, &c.; but, to enter into all theſe details, would require too much time.

In regard to the columnar apparatus, I endeavoured to diſcover the means of lengthening it a great deal by multiplying the metallic plates in ſuch a manner as not to tumble down; and I diſcovered, beſides others, the following, which are repreſented in the annexed figures. (Plate VIII. fig. 2, 3, 4.)

In Fig. 2, *mmmm* are rods, three, four, or more in number, which riſe from the bottom of the column, and confine, as in a cage, the plates or diſks, placed each above the other in ſuch number and to ſuch a height as you chooſe, and which thus prevent them from falling. The rods may be of glaſs, wood, or metal, only that, in the laſt caſe, you muſt prevent them from coming into immediate contact with the plates; which may be done either by covering each of them with a glaſs tube, or interpoſing between them and the column a few ſtripes of wax cloth, oiled paper, or even plain paper, and, in a word, any other body that may either be a *cohibent* or a bad conductor: wood or paper will be ſufficiently ſo for our purpoſe, provided only that they are not very damp or moiſt.

But the beſt expedient, when you wiſh to form an apparatus to conſiſt of a great number of plates, above 60, 80, or 100 for example, is, to divide the column into two or more, as ſeen Fig. 3 and 4, (Plate VIII.) where the pieces all have their reſpective poſitions and communication as if there were only one column. Fig. 4, as well as Fig. 3, may indeed be conſidered as a bent column.

In all theſe figures the different metallic plates are denoted by the letters S and Z (which are the initials of ſilver and zinc); and the *moiſtened diſks* (of paſteboard, ſkin, &c. interpoſed between each pair of metals), are repreſented by a black ſtratum. The plates of metal may either be laid ſimply upon each other and ſo brought into union in an indefinite number of points, or they may be ſoldered together. It is altogether indifferent whichever of theſe methods be followed. *cc*, *cc*, *cc*, are the metallic plates which form a communication between each column, or ſection of a column, and another; and *bb*, *bb*, *bb*, are the baſons of water in communication with the lower part or extremities of theſe columns.

An apparatus thus prepared is exceedingly convenient without being bulky; and it might be rendered portable, with ſtill more eaſe and ſafety, by means of circular caſes or tubes, in which each column might be incloſed and preſerved. It is only to be regretted that it does not long continue in a good ſtate: the moiſtened diſks become dry in one or two days to ſuch a degree that they muſt be again moiſtened; which, however, may be done without taking to pieces the whole apparatus, by immerſing the columns, completely formed, in water, and wiping them, when taken out ſome time after, with a cloth, or in any other manner.

The beſt method of making an inſtrument as durable as can be wiſhed, would be, to incloſe and confine the water interpoſed between each pair of metals, and to fix theſe metallic plates in their places by enveloping the whole column with wax or pitch: but this would be ſomewhat difficult in the execution, and would require a great deal of patience. I have, however, ſucceeded; and have formed in this manner two cylinders conſiſting of twenty pair of metals, which can ſtill be employed though made ſeveral weeks, and which, I hope, will be ſerviceable for months.

Theſe cylinders are attended with this advantage, that they may be employed for experiments either in an erect, inclined, or lying poſition, according as you chooſe, or even immerſed in water, provided the top of it be above the ſurface of the fluid: they might alſo give a ſhock when entirely immerſed if they contained a greater number of plates, or if ſeveral of theſe cylinders were joined together, and if there were any interruption that could be removed at pleaſure, &c. by which means theſe cylinders would have a pretty good reſemblance to the electric eel; and, to have a better reſemblance to it even externally, they might be joined together by pliable metallic wires or ſcrew-ſprings, and then covered with a ſkin terminated by a head and tail properly formed, &c.

The effects ſenſible to our organs produced by an apparatus formed of 40 or 50 pair of plates (and even by a ſmaller, if one of the metals be ſilver or copper and the other zinc,) are reduced merely to ſhocks: the current of the electric fluid, impelled and excited by ſuch a number and variety of different conductors, ſilver, zinc, and water, diſpoſed alternately in the manner above deſcribed, excites not only contractions and ſpaſms in the muſcles, convulſions more or leſs violent in the limbs through which it paſſes in its courſe; but it irritates alſo the organs of taſte, ſight, hearing, and feeling, properly ſo called, and produces in them ſenſations peculiar to each.

And firſt, in regard to the ſenſe of feeling: If, by means of an ample contact of the hand (well moiſtened) with a plate of metal, or rather, by immerſing the hand to a conſiderable depth in the water of the baſon, I eſtabliſh on one ſide a good communication with one of the extremities of my *electro-motive* apparatus, (we muſt give new names to inſtruments that are new not only in their form, but in their effects or the principle on which they depend); and on the other I apply the forehead, eye-lid, tip of the noſe, alſo well moiſtened, or any other part of the body where the ſkin is very delicate: if I apply, I ſay, with a little preſſure, any one of theſe delicate parts, well moiſtened, to the point of a metallic wire, communicating properly with the other extremity of the ſaid apparatus, I experience, at the moment that the conducting circle is completed, at the place of the ſkin touched, and a little beyond it, a blow and a prick, which ſuddenly paſſes, and is repeated as many times as the circle is interrupted and reſtored; ſo that, if theſe alternations be frequent, they occaſion a very diſagreeable quivering and pricking. But if all theſe communications continue without theſe alternations, without the leaſt interruption of the circle, I feel nothing for ſome moments; afterwards, however, there begins at the part applied to the end of the wire, another ſenſation, which is a ſharp pain (without ſhock), limited preciſely by the points of contact, a quivering, not only continued, but which always goes on increaſing to ſuch a degree, that in a little time it becomes inſupportable, and does not ceaſe till the circle is interrupted.

What proof more evident of the continuation of the electric current as long as the communication of the conductors forming the circle is continued?—and that ſuch a current is only ſuſpended by interrupting that communication? This endleſs circulation of the electric fluid (this *perpetual motion*) may appear paradoxical and even inexplicable, but it is no leſs true and real; and you feel it, as I may ſay, with your hands. Another evident proof may be drawn from this circumſtance, that in ſuch experiments you often experience, at the moment when the circle is ſuddenly interrupted, a ſhock, a pricking, an agitation, according to circumſtances, in the ſame manner as at the moment when it is completed; with this only difference, that theſe ſenſations, occaſioned by a kind of reflux of the electric fluid, or by the ſhock which ariſes from the ſudden ſuſpenſion of its current, are of leſs ſtrength. But I have no need, and this is not the place to bring forward proofs of ſuch an endleſs circulation of the electric fluid in a circle of conductors, where there are ſome, which, by being of a different kind, perform, by their mutual contact, the office of exciters or *movers:* this propoſition, which I advanced in my firſt reſearches and diſcoveries on the ſubject of galvaniſm, and always maintained by ſupporting them with new facts and experiments, will, I hope, meet with no oppoſers.

Recurring to the ſenſation of pain which is felt in the experiments above deſcribed, I muſt add, that if this pain be very ſtrong and pricking in the parts covered by the ſkin, it is much more ſo in thoſe where the ſkin has been taken off—in recent wounds for example. If by chance there ſhould be a ſmall inciſion or bit of the ſkin rubbed off in the finger which I immerſe in the water that communicates with one of the extremities of the *electro-motive* apparatus, I experience there a pain ſo acute, when, by eſtabliſhing the proper communication with the other extremity, I complete the circle, that I muſt ſoon deſiſt from the experiment; that is to ſay, muſt withdraw my finger, or interrupt the circle in ſome other manner. I will ſay more; that I cannot even endure it above a few ſeconds when the part of the apparatus which I put in play, or the whole apparatus, contains only twenty pair of plates, or about that number.

One thing, which I muſt ſtill remark, is, that all theſe ſenſations of pricking and pain are ſtronger and ſharper, every thing elſe being equal, when the part of the body which is to feel them is towards the negative electricity; that is to ſay, placed in ſuch a manner in the conducting circle, that the electric fluid traverſing that circle is not directed towards that ſenſible part, does not advance towards it, and enter from the outſide inwards, but takes its direction from the inſide outwards; in a word, that it iſſues from it: in regard to which it is neceſſary to know, of the two metals that enter by pairs into the conſtruction of the machine, which is the one that gives off to the other. But I had already determined this reſpecting all the metals by other experiments, publiſhed a long time ago at the end of my firſt memoirs on galvaniſm. I ſhall therefore ſay nothing further here, than that the whole is completely confirmed by the experiments, equally and ſtill more demonſtrative and ſtriking, with which I am at preſent employed.

In regard to the ſenſe of taſte, I had before diſcovered, and publiſhed in theſe firſt memoirs, where I found myſelf obliged to combat the pretended animal electricity of Galvani, and to declare it an external electricity moved by the mutual contact of metals of different kinds,—I had diſcovered, I ſay, in conſequence of this power which I aſcribed to metals, that two pieces of theſe different metals, and particularly one of ſilver and one of zinc, applied in a proper manner, excited at the tip of the tongue very ſenſible ſenſations of taſte; that the taſte was decidedly acid, if, the tip of the tongue being turned towards the zinc, the electric current proceeded againſt it, and entered it; and that another taſte, leſs ſtrong but more diſagreeable, acrid, and inclining to alkaline, was felt, if (the poſition of the metals being reverſed) the electric current iſſued from the tip of the tongue; that theſe ſenſations continued and received even an increaſe for ſeveral ſeconds, if the mutual contact of the two metals was maintained, and if the conducting circle was nowhere interrupted. But when I have ſaid here, that exactly the ſame phenomena take place when you try, inſtead of one pair of theſe metallic pieces, an aſſemblage of ſeveral of them ranged in the proper manner; and that the ſaid ſenſations of taſte, whether acid or alkaline, increaſe but a little with the number of theſe pairs, I have ſaid the whole. It only remains for me to add that, if the apparatus put in play for theſe experiments on the tongue be formed of a ſufficiently large number of metallic pairs of this kind, for example, if it contain 30, 40, or more, the tongue experiences not only the ſenſation of taſte already mentioned, but, beſides that, a blow which it receives at the moment when the circle is completed, and which occaſions in it a pricking more or leſs painful, but fleeting, followed ſome moments after by a durable ſenſation of taſte. This blow produces even a convulſion or agitation of a part or of the whole of the tongue, when the apparatus, formed of a ſtill greater number of pairs of the ſaid metals, is more active, and if, by means of good communicating conductors, the electric current which it excites be able to paſs every where with perfect freedom.

I muſt often recur to, and inſiſt on, this laſt condition, becauſe it is eſſential in all experiments when you wiſh to obtain ſenſible effects on the body, or commotions in the limbs, or ſenſations in the organs of the ſenſes. It is neceſſary, therefore, that the non-metallic conductors which enter into the circle ſhould be as good conductors as poſſible, well moiſtened (if they are not themſelves liquid) with water, or with any other liquid that may be a better conductor than pure water; and it is neceſſary, beſides, that the well moiſtened ſurfaces, by which they communicate with the metallic conductor, ſhould be ſufficiently large. The communication ought to be confined or reduced to a ſmall number of points of contact only in that place where you wiſh to concentrate the electric action on one of the moſt ſenſible parts of the body, on any of the ſenſitive nerves, &c. as I have already remarked in ſpeaking of the experiments on feeling, *viz.* thoſe by which acute pains are excited in different parts. The beſt method which I have found for producing on the tongue all the ſenſations above deſcribed, is, to apply the tip of it to the pointed extremity (which, however, muſt not be too much ſo) of a metallic rod, which I make to communicate properly, as in the other experiments, with one of the extremities of my apparatus, and to eſtabliſh a good communication between the hand, or, what is better, both the hands together, and the other extremity. This application of the tip of the tongue to the end of the metallic rod, may either exiſt already, when you are going to make the other communication to complete the circle (when you are going to immerſe your hand into the water of the baſon), or be made after the eſtabliſhment of this communication, while the hand is immerſed; and in the latter caſe I think I feel the pricking and ſhock in the tongue, a very ſhort time before actual contact. Yes; it always appears to me, particularly if I advance the tip of my tongue gradually, that, when it has arrived within a very ſmall diſtance of the metal, the electric fluid (I would almoſt ſay ſpark), overcoming this interval, darts forwards to ſtrike it.

In regard to the ſenſe of ſight, which I alſo found might be affected by the weak current of the electric fluid, ariſing from the mutual contact of two different metals in general, and in particular, of a piece of ſilver and one of zinc, it was natural to expect that the ſenſation of light, excited by my new apparatus, would be ſtronger in proportion as it contained a greater number of pieces of theſe metals; each pair of which, arranged in the proper manner, adds a degree of force to the ſaid electric current, as all the other experiments ſhow, and particularly thoſe with the electrometer aſſiſted by the condenſer, which I have only mentioned, and which I ſhall deſcribe on another occaſion. But I was ſurpriſed to find that, with 10, 20, 30 pairs, and more, the flaſh produced neither appeared longer and more extended, nor much brighter than with one pair. It is true, however, that this ſenſation of weak and tranſient light, is excited by ſuch an apparatus much eaſier and in different ways. To ſucceed, indeed, with one pair, the following are almoſt the only methods; *viz.* that one of the metallic pieces ſhould be applied to the ball of the eye, or the eye-lid well moiſtened, and that it ſhould be made to touch the other metal applied to the other eye, or held in the mouth, which produces a a flaſh much more beautiful; or, that this ſecond metallic piece ſhould be held in the moiſtened hand and then brought into contact with the former; or, in the laſt place, that theſe two plates ſhould be applied to certain parts of the inſide of the mouth, making them communicate with each other. But with an apparatus of 20 or 30 pairs, &c. the ſame flaſh will be produced by applying the end of a metallic plate or rod, placed in communication with one of the extremities of the apparatus, to the eye, while with one hand you form a proper communication with the other extremity; by bringing, I ſay, this plate into contact not only with the eye or any part of the mouth, but even the forehead, the noſe, the cheeks, lips, chin, and even the throat; in a word, every part and point of the viſage, which muſt only be well moiſtened before they are applied to the metallic plate. The form as well as the force of this tranſient light which is perceived varies a little, if the places of the face to which the action of the electric current is applied, be varied: if it be on the forehead, for example, this light is moderately bright, and appears like a luminous circle, under which ſigure it preſents itſelf alſo in ſeveral other experiments.

But the moſt curious of all theſe experiments is, to hold the metallic plate between the lips, and in contact with the tip of the tongue; ſince, when you afterwards complete the circle in the proper manner, you excite at once, if the apparatus be ſufficiently large and in good order, and the electric current ſufficiently ſtrong and in good order, a ſenſation of light in the eyes, a convulſion in the lips, and even in the tongue, and a painful prick at the tip of it, followed by a ſenſation of taſte.

I have now only to ſay a few words on hearing. This ſenſe, which I had in vain tried to excite with only two metallic plates, though the moſt active of all the *exciters* of electricity, *viz.* one of ſilver or gold, and the other of zinc, I was at length able to affect it with my new apparatus, compoſed of 30 or 40 pairs of theſe metals. I introduced, a conſiderable way into both ears, two probes or metallic rods with their ends rounded, and I made them to communicate immediately with both extremities of the apparatus. At the moment when the circle was thus completed I received a ſhock in the head, and ſome moments after (the communication continuing without any interruption) I began to hear a ſound, or rather noiſe, in the ears, which I cannot well define: it was a kind of crackling with ſhocks, as if ſome paſte or tenacious matter had been boiling. This noiſe continued inceſſantly, and without increaſing, all the time that the circle was complete, &c. The diſagreeable ſenſation, and which I apprehended might be dangerous, of the ſhock in the brain, prevented me from repeating this experiment.

There ſtill remains the ſenſe of ſmelling, which I have hitherto tried in vain with my apparatus. The electric fluid, which, when made to flow in a current in a complete circle of conductors, produces in the limbs and parts of the living body effects correſpondent to their excitability, which ſtimulating in particular the organs or nerves of touch, taſte, ſight, and hearing, excite in them ſome ſenſations peculiar to each of theſe ſenſes, as I have found, produces in the interior of the noſe only a pricking more or leſs painful, and commotions more or leſs extenſive, according as the ſaid current is weaker or ſtronger. And whence comes it, then, that it does not excite any ſenſation of ſmell, though, as appears, it ſtimulates the nerves of that ſenſe? It cannot be ſaid that the electric fluid of itſelf is not proper for producing odorous ſenſations, ſince, when it diffuſes itſelf through the air in the form of aigrettes, &c. in the common experiments made with electric machines, it conveys to the noſe a very ſenſible ſmell reſembling that of phoſphorus. Taking ſimilitude into conſideration, and reaſoning from its analogy with other odoriferous matters, I will ſay, that it muſt completely diffuſe itſelf throughout the air to excite ſmell; that it has need, like other effluvia, of the vehicle of the air to affect that ſenſe in ſuch a manner as to excite the ſenſations of ſmell. But in the experiments of which I ſpeak, that is to ſay, of an electric current in a circle of conductors, all contiguous, and without the leaſt interruption, this abſolutely cannot take place.

All the facts which I have related in this long paper in regard to the action which the electric fluid excited, and when moved by my apparatus, exerciſes on the different parts of our body which the current attacks and paſſes through;—an action which is not momentaneous, but which laſts, and is maintained during the whole time that this current can follow the chain not interrupted in its communications; in a word, an action the effects of which vary according to the different degrees of excitability in the parts, as has been ſeen;—all theſe facts, ſufficiently numerous, and others which may be ſtill diſcovered by multiplying and varying the experiments of this kind, will open a very wide field for reflection, and of views, not only curious, but particularly intereſting to medicine. There will be a great deal to occupy the anatomiſt, the phyſiologiſt, and the practitioner.

It is well known, by the anatomy which has been made of it, that the electric organ of the torpedo or electric eel, conſiſts of ſeveral membranaceous columns, filled from one end to the other with a great number of plates or pellicles, in the form of very thin diſks, placed one upon the other, or ſupported at very ſmall diſtances by intervals, into which, as appears, ſome liquor flows. But we cannot ſuppoſe that any of theſe laminæ are of an inſulating nature, like glaſs, reſin, ſilk, &c. and ſtill leſs that they can either become electric by friction, or be diſpoſed and charged in the ſame manner as the ſmall Franklinian plates or ſmall electrophores; nor even that they are ſufficiently bad conductors to perform the office of a good and durable condenſer, as Mr. Nicholſon has ſuppoſed. The hypotheſis of this learned and laborious philoſopher, by which he makes of each pair of theſe pellicles, which he compares to leaves of talc, as many ſmall *electrophores* or *condenſers*, is indeed very ingenious, and is, perhaps, the beſt theory that has been deviſed to explain the phenomena of the torpedo, adhering to the hitherto known principles and laws of electricity. For, beſides that the mechaniſm, by which, every time that the fiſh intended to give a ſhock, the reſpective ſeparation of the plates of the whole or a great number of theſe electrophores or condenſers ought to be effected all at once, and ought to eſtabliſh on the one hand a communication between themſelves of all the plates electrified *poſitively*, and on the other a communication between all thoſe electrified *negatively*, as Mr. Nicholſon ſuppoſes—beſides, that this very complex mechaniſm appears too difficult, and little agreeable to nature;—and beſides, that the ſuppoſition of an electric charge originally impreſſed, and ſo durable in theſe pellicles performing the office of electrophores, is altogether gratuitous,—ſuch a hypotheſis falls entirely, ſince theſe pellicles of the organ of the torpedo are not, and cannot be, in any manner inſulating or ſuſceptible of a real electric charge, and much leſs capable of retaining it. Every animal ſubſtance, as long as it is freſh, ſurrounded with juices, and more or leſs ſucculent of itſelf, is a very good conductor. I ſay more, inſtead of being as *cohibent* as reſins or talc, to leaves of which Mr. Nicholſon has compared the pellicles in queſtion, there is not, as I have aſſured myſelf, any living or freſh animal ſubſtance which is not a better *deferent* than water, except only greaſe and ſome oily humours. But neither theſe humours nor greaſe, eſpecially ſemi-fluid or entirely fluid, as it is found in living animals, can receive an electric charge in the manner of inſulating plates, and retain it: beſides, we do not find that the pellicles and humours of the organ of the torpedo are greaſy or oily. This organ therefore, compoſed entirely of conducting ſubſtances, cannot be compared either to the electrophore or condenſer, or to the Leyden flaſk, or any machine excitable by friction or by any other means capable of electrifying inſulating bodies, which before my diſcoveries were always believed to be the only ones originally electric.

To what electricity then, or to what inſtrument ought the organ of the torpedo or electric eel, &c. to be compared? To that which I have conſtructed according to the new principle of electricity, diſcovered by me ſome years ago, and which my ſucceſſive experiments, particularly thoſe with which I am at preſent engaged, have ſo well confirmed, *viz.* that conductors are alſo, in certain caſes, exciters of electricity in the caſe of the mutual contact of thoſe of different kinds, &c. in that apparatus which I have named the *artificial electric organ*, and which being at bottom the ſame as the natural organ of the torpedo, reſembles it alſo in its form, as I have advanced.

II. *Inveſtigation of the Powers of the Priſmatic Colours to Heat and Illuminate Objects; with Remarks that prove the different Refrangibility of Radiant Heat: to which is added, an Inquiry into the Method of viewing the Sun advantageouſly, with Teleſcopes of large Apertures and high Magnifying Powers.* By William Herschel, *LL.D. F.R.S.*[*]

It is ſometimes of great uſe in natural philoſophy to doubt of things that are commonly taken for granted; eſpecially as the means of reſolving any doubt, when once it is entertained, are often within our reach. We may therefore ſay that any experiment which leads us to inveſtigate the truth of what was before admitted upon truſt may become of great utility to natural knowledge. Thus, for inſtance, when we ſee the effect of the condenſation of the ſun's rays in the focus of a burning lens, it ſeems to be natural to ſuppoſe that every one of the united rays contributes its proportional ſhare to the intenſity of the heat which is produced; and we ſhould probably think it highly abſurd, if it were aſſerted that many of them had but little concern in the combuſtion or vitrification which follows, when an object is put into that focus. It will therefore not be amiſs to mention what gave riſe to a ſurmiſe, that the power of heating and illuminating objects might not be equally diſtributed among the variouſly coloured rays.

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[*] From *Tranſactions of the Royal Society of London for* 1800.

In a variety of experiments which I have occaſionally made, relating to the method of viewing the ſun with large teleſcopes to the beſt advantage, I uſed various combinations of differently-coloured darkening glaſſes. What appeared remarkable was, that when I uſed ſome of them I felt a ſenſation of heat, though I had but little light; while others gave me much light, with ſcarce any ſenſation of heat. Now, as in theſe different combinations the ſun's image was alſo differently coloured, it occurred to me, that the priſmatic rays might have the power of heating bodies very unequally diſtributed among them; and, as I judged it right in this reſpect to entertain a doubt, it appeared equally proper to admit the ſame with regard to light. If certain colours ſhould be more apt to occaſion heat, others might, on the contrary, be more fit for viſion, by poſſeſſing a ſuperior illuminating power. At all events, it would be proper to recur to experiments for a deciſion.

Experiments on the heating Power of coloured Rays.

I fixed a piece of paſteboard, AB, (Plate IX.) in a frame, mounted upon a ſtand, CD, and moveable upon two centres. In the paſteboard I cut an opening, *mn*, a little larger than the ball of a thermometer, and of a ſufficient length to let the whole extent of one of the priſmatic colours paſs through. I then placed three thermometers upon ſmall inclined planes, EF; their balls were blacked with japan ink. That of No. 1 was rather too large for great ſenſibility. No. 2 and 3 were two excellent thermometers, which my highly eſteemed friend Dr. Wilſon, late profeſſor of aſtronomy at Glaſgow, had lent me for the purpoſe: their balls being very ſmall, made them of exquiſite ſenſibility. The ſcales of all were properly diſengaged from the balls.

I now placed the ſtand, with the framed paſteboard and the thermometers, upon a ſmall plain board, GH, that I might be at liberty to move the whole apparatus together, without deranging the relative ſituation of the different parts.

This being done, I ſet a priſm, moveable on its axis, into the upper part of an open window, at right angles to the ſolar ray, and turned it about till its refracted coloured ſpectrum became ſtationary upon a table placed at a proper diſtance from the window.

The board containing the apparatus was now put on the table, and ſet in ſuch a manner as to let the rays of one colour paſs through the opening in the paſteboard. The moveable frame was then adjuſted to be perpendicular to the rays coming from the priſm; and the inclined planes carrying the three thermometers, with their balls arranged in a line, were ſet ſo near the opening, that any one of them might eaſily be advanced far enough to receive the irradiation of the colour which paſſed through the opening, while the reſt remained cloſe by, under the ſhade of the paſteboard.

By repeated trials, I found that Dr. Wilſon's No. 2, and mine, always agreed in ſhowing the temperature of the place where I examined them, when the change was not very ſudden; but that mine would require ten minutes to take a change, which the other would ſhow in five. No. 3 never differed much from No. 2.

1ſt Experiment. Having arranged the three thermometers in the place prepared for the experiment, I waited till they were ſtationary. Then, advancing No. 1. to the red rays, and leaving the other two cloſe by, in the ſhade, I marked down what they ſhowed at different times.

No. 1.	No. 2.	No. 3.	
43½	43½	43¼	This, in about 8 or 10 minutes, gave 6¾ degrees, for the riſing produced in my thermometer by the red rays, compared to the two ſtandard thermometers.
48	43½	43½	
49½	43¼	43¼	
49¾	43¼	43¼	
50	43¼	43¼	

2d Experiment. As ſoon as my thermometer was reſtored to the temperature of the room, which I haſtened, by applying it to a large piece of metal that had been kept in the ſame place, I expoſed it again to the red rays, and regiſtered its march, along with No. 2 as a ſtandard, which was as follows:

No. 1.	No. 2.	
45	45	Hence, in 10 minutes, the red rays made the thermometer riſe 7 degrees.
48	45	
51	45	
51	44½	
51	44	

3d Experiment. Proceeding in the ſame manner as before, in the green rays I had,

No. 1.	No. 2.	
43	43	Therefore, in 10 minutes, the green rays occaſioned a riſe of 3¼ degrees.
45½	43	
46	43	
46	42¾	
46	42¾	

4th Experiment. I now expoſed my thermometer to the violet rays, and compared it with No. 2.

No. 1.	No. 2.	
44	44	Here we have a riſing of 2 degrees in 10 minutes, for the violet rays.
44	44	
44¾	43½	
45	43	

5th Experiment. I now expoſed Dr. Wilſon's thermometer No. 2 to the red rays, and compared its progreſs with No. 3.

No. 2.	No. 3.	
44	44	Here the thermometer, expoſed to red, roſe in five minutes 2¾ degrees.
46	44	
46½	43¾	
46¾	43¾	

6th Experiment. In red rays again.

No. 2.	No. 3.	
44	44	And here the thermometer, expoſed to red, roſe in five minutes 4 degrees.
46	44	
46½	43½	
47	43½	
47	43	

7th Experiment. In green rays.

No. 2.	No. 3.	
43½	43½	This made the thermometer riſe, in the green rays, 1½ degree.
44½	43½	
44½	43	

8th Experiment. Again in green rays.

No. 2.	No. 3.	
43	43	Here the riſing by the green rays, was 2 degrees.
44½	42¼	
44¾	42¾	

From theſe experiments we are authoriſed to draw the following reſults: in the red rays my thermometer gave 6¾ degrees in the 1ſt, and 7 degrees in the 2d, for the riſing of the quickſilver: a mean of both is 6⅞. In the 3d experiment we had 3¼ degrees, for the riſing occaſioned by the green rays; from which we obtain the proportion of 55 to 26, for the power of heating in red to that in green. The 4th experiment gave 2 degrees for the violet rays; and therefore we have the riſing of the quickſilver in red to that in violet, as 55 to 16.

A ſufficient proof of the accuracy of this determination we have in the reſult of the four laſt experiments. The riſing for red rays in the 5th, is 2¾; and in the 6th, 4 degrees: a mean of both is 3⅜. In the 7th experiment we have 1½, and in the 8th 2 degrees, for the riſing in the green: a mean of theſe is 1¾. Therefore, we have the proportion of the riſing in red to that in green, as 27 to 11, or as 55 to 22,4.

We may take a mean of the reſult of both thermometers, which will be 55 to 24,2, or more than 2¼ to 1, in red to green; and about 3½ to 1, in red to violet.

It appears remarkable, that the moſt ſenſible thermometer ſhould give the leaſt alteration, from the expoſure to the coloured rays. But ſince, in theſe circumſtances, there are two cauſes conſtantly acting different ways: the one to raiſe the thermometer, the other to bring it down to the temperature of the room, I ſuppoſe that, on account of the ſmallneſs of the ball in Dr. Wilſon's No. 1, which is but little more than $\frac{1}{6}$th of an inch, the cooling cauſes muſt have a ſtronger effect on the mercury which it contains than they can have on mine, the ball of which is half an inch.

More accuracy may hereafter be obtained by attending to the circumſtances of blacking the balls of the thermometers, and their expoſure to a more ſteady and powerful light of the ſun, at greater altitudes than it can be had at preſent; but the experiments which have been related are quite ſufficient for my preſent purpoſe; which only goes to prove that the heating power of the priſmatic colours is very far from being equally divided, and that the red rays are chiefly eminent in that reſpect.

Experiments on the illuminating Power of coloured Rays.

In the following examination of the illuminating power of differently coloured rays, I had two ends in view. The firſt was with regard to the illumination itſelf, and the next with reſpect to the aptneſs of the rays for giving diſtinct viſion; and, though there did not ſeem to be any particular reaſon why theſe two ſhould not go together, I judged it right to attend to both.

The microſcope offered itſelf as the moſt convenient inſtrument for this inveſtigation; and I thought it expedient to view only opaque objects, as theſe would give me an opportunity to uſe a direct priſmatic ray, without running the riſk of any bias that might be given to it, in its tranſmiſſion through the colouring particles of tranſparent objects.

1ſt Experiment. I placed an object that had very minute parts under a double microſcope; and, having ſet a priſm in the window, ſo as to make the coloured image of the ſun ſtationary upon the table where the microſcope was placed, I cauſed the differently coloured rays to fall ſucceſſively on the object by advancing the microſcope into their light. The magnifying power was 27 times.

In changing the illumination, by admitting a different colour, it always becomes neceſſary to re-adjuſt the inſtrument. It is well known, that the different refrangibility of the rays will ſenſibly affect the focal length of object-glaſſes; but in compound viſion, ſuch as in a microſcope, where a very ſmall lens is made to caſt a lengthened ſecondary focus, this difference becomes ſtill more conſiderable.

By an attentive and repeated inſpection, I found that my object was very well ſeen in red, better in orange, and ſtill better in yellow; full as well in green; but to leſs advantage in blue; indifferently well in indigo, and with more imperfection in violet.

The trial was made upon one of the microſcopic objects which are generally prepared for tranſparent viſion; but as I uſed it in the opaque way, I thought that others might be choſen which would anſwer the purpoſe better; and, in order to give ſome variety to my experiments, and to ſee the effect which differently coloured ſubſtances might have on the rays of light, I provided the following materials to be viewed:—red paper, green paper, a piece of braſs, a nail, a guinea, black paper. Having alſo found that a higher power might be uſed, with ſufficient convenience for the rays of light to come from the priſm to the object, I made the microſcope magnify 42 times.

The appearance of the nail in the microſcope is ſo beautiful, that it deſerves to be noticed; and the more ſo, as it is accompanied with circumſtances that are very favourable for an inveſtigation, ſuch as that which is under our preſent conſideration. I had choſen it on account of its ſolidity and blackneſs, as being moſt likely to give an impartial reſult of the modifications ariſing from an illumination by differently coloured rays; but, on viewing it, I was ſtruck with the ſight of a bright conſtellation of thouſands of luminous points ſcattered over its whole extent, as far as the field of the microſcope could take it in. Their light was that of the illuminating colour, but differed conſiderably in brightneſs; ſome of the points being dim and faint, while others were luminous and brilliant. The brighteſt of them alſo admitted of a little variation in their colour, or rather in the intenſity of the ſame colour; for in the centre of ſome of the moſt brilliant of theſe lucid appearances, their light had more vivacity, and ſeemed to deviate from the illuminating tint towards whiteneſs, while on and near the circumference, it appeared to take a deeper hue.

An object ſo well divided by nature into very minute and differently arranged points, on which the attention might be fixed, in order to aſcertain whether they would be equally diſtinct in all colours, and whether their number would be increaſed or diminiſhed by different degrees of illumination, was exactly what I wanted; nor could I think it leſs remarkable, that all the other objects I had fixed upon, beſides many more which have been examined, ſuch as copper, tin, ſilver, &c. preſented themſelves nearly with the ſame appearance. In the braſs, which had been turned in a lathe, the luminous points were arranged in furrows; and in tin they were remarkably beautiful. The reſult of the examination of my objects was as follows:

2d Experiment. Red paper.—In the red rays I view a bright point near an accidental black ſpot in the paper, which ſerves me as a mark; and I notice the ſpace between the point and the ſpot; it contains ſeveral faint points: In the orange rays I ſee better; the bright point I now perceive is double: In the yellow rays, I ſee the object ſtill better: In the green rays, full as well as before: In the blue rays, very well: In the indigo rays, not quite ſo well as in the blue: In the violet rays very imperfectly.

3d Experiment. Green paper.—*Red.* I fix my attention on many faint ſpots in a ſpace between two bright double points.—*Orange.* I ſee thoſe faint points better.—*Yellow.* Still better.—*Green.* As well as before; I ſee remarkably well.—*Blue.* Leſs bright, but very diſtinct.—*Indigo.* Not well.—*Violet.* Bad.

4th Experiment. A piece of very clean turned braſs.—*R.* I remark ſeveral faint luminous points between two bright ones; the colour of the braſs makes the red rays appear like orange.—*O.* I ſee better, but the orange colour is likewiſe different from what it ought to be; however, this is not at preſent the object of my inveſtigation.—*Y.* I ſee ſtill better.—*G.* I ſee full as well as before.—*B.* I do not ſee ſo well now.—*I.* I cannot ſee well.—*V.* Bad.

5th Experiment. A nail.—*R.* I remark two bright points and ſome faint ones.—*O.* Brighter than before, and more points viſible; very diſtinct.—*Y.* Much brighter than before, and more points and lines viſible; very diſtinct.—*G.* Full as bright, and as many points viſible; very diſtinct.—*B.* Much leſs bright; very diſtinct.—*I.* Still leſs bright; very diſtinct.—*V.* Much leſs bright again; very diſtinct.

6th Experiment. I viewed a guinea at 9 feet 6 inches from the priſm, and adjuſted the place of the object in the ſeveral rays by the ſhadow of the guinea; if this be not done, deceptions will take place.—*R.* Four remarkable points; very diſtinct.—*O.* Better illuminated; very diſtinct.—*Y.* Still better illuminated; very diſtinct. The points all over the field of view are coloured; ſome green, ſome red, ſome yellow, and ſome white encircled with black about them; between yellow and green is the maximum of illumination. Extremely diſtinct.—*G.* As well illuminated as the yellow; very diſtinct.—*B.* Much inferior in illumination; very diſtinct.—*I.* Badly illuminated; diſtinct.—*V.* Very badly illuminated: I can hardly ſee the object at all.

7th Experiment. The nail again at 8 feet from the priſm.—*R.* I attended to two bright points, with faint ones between them; almoſt all the points in the field of view are red; very diſtinct.—*O.* I ſee all the points better; they are red, green, yellow, and whitiſh, with black about them; very diſtinct.—*Y.* I ſee better; more bright points and more faint ones; the points are of various colours; very diſtinct.—*G.* I ſee as well; the points are moſtly green and brightiſh-green, inclining to white; very diſtinct.—*B.* Much worſe illuminated; very diſtinct.—*I.* Badly illuminated; very diſtinct.—*V.* There is hardly any illumination.

8th Experiment. The nail again at 9 feet 6 inches from the priſm, by way of having the rays better ſeparated.—*R.* Badly illuminated; the bright points are very diſtinct.—*O.* Much better illuminated; the bright points very diſtinct. *Y.* Still better illuminated; all points extremely diſtinct.—*G.* As well illuminated, and equally diſtinct.—*B.* Badly illuminated; the bright points are diſtinct, but the others are not ſo.—*I.* Very badly illuminated; I do not ſee diſtinctly; but I believe it to be for want of light.—*V.* So badly illuminated that I cannot ſee the object; or at leaſt but barely perceive that it exiſts.

9th Experiment. Black paper at 8 feet from the priſm.—*R.* The object is hardly viſible; I can only ſee a few faint points.—*O.* I ſee ſeveral bright points and many faint ones.—*Y.* Numberleſs bright and ſmall faint points; between yellow and green is the maximum of illumination.—*G.* The ſame as the yellow.—*B.* Very indifferently illuminated; but not ſo bad as in the red rays.—*I.* I cannot ſee the object.—*V.* Totally inviſible.

From theſe obſervations, which agree uncommonly well with reſpect to the illuminating power aſſigned to each colour, we may conclude that the red-making rays are very far from having it in any eminent degree. The orange poſſeſs more of it than the red; and the yellow rays illuminate objects ſtill more perfectly. The maximum of illumination lies in the brighteſt yellow, or paleſt green. The green itſelf is nearly equally bright with the yellow; but, from the full deep green, the illuminating power decreaſes very ſenſibly. That of the blue is nearly upon a par with that of the red; the indigo has much leſs than the blue; and the violet is very deficient.

With regard to the principle of diſtinctneſs, there appears to be no deficiency in any one of the colours. In the violet rays, for inſtance, ſome of the experiments mention that I ſaw badly; but this is to be underſtood only with reſpect to the number of ſmall objects that could be perceived; for, although I ſaw fewer of the points, thoſe which remained viſible were always as diſtinct as, in ſo feeble an illumination, could be expected. It muſt indeed be evident that, by removing the great obſtacle to diſtinct viſion, which is the different refrangibility of the rays of light, a microſcope will be capable of a much higher degree of diſtinctneſs than it can be under the uſual circumſtances. A celebrated optical writer has formerly remarked that a fly, illuminated by red rays, appeared uncommonly diſtinct, and that all its minute parts might be ſeen in great perfection; and, from the experiments which have been related, it appears that every other colour is poſſeſſed of the ſame advantage.

I am well aware that the reſults I have drawn from the foregoing experiments, both with regard to the heating and illuminating powers of differently coloured rays, muſt be affected by ſome little inaccuracies. The priſm, under the circumſtances in which I have uſed it, could not effect a complete ſeparation of the colours; on account of the apparent diameter of the ſun, and the conſiderable breadth of the priſm itſelf, through which the rays were tranſmitted.

Perhaps an arrangement like that in Fig. 16. of the Newtonian experiments might be employed, if inſtruments of ſufficient ſenſibility, ſuch as air thermometers, can be procured, that may be affected by the enfeebled illumination of rays that have undergone four tranſmiſſions and eight refractions; and eſpecially when their incipient quantity has been ſo greatly reduced in their limited paſſage through a ſmall hole at the firſt incidence.

But it appeared moſt expedient for me, at preſent, to neglect all further refinements, which may be attempted hereafter at leiſure. It may even be preſumed that, had there not been ſome ſmall admixture of the red rays in the other colours, the reſult would have been ſtill more deciſive with regard to the power of heating veſted in the red rays. And it is likewiſe evident, that at leaſt the red light of the priſmatic ſpectrum was much leſs adulterated than any of the other colours; their refractions tending all to throw them from the red. That the ſame rays which occaſion the greateſt heat, have not the power of illumination in any ſtrong degree, ſtands on as good a foundation. For, ſince here alſo they have undergone the faireſt trial, as being moſt free from other colours, it is equally proved that they illuminate objects but imperfectly. There is ſome probability that a ray, purified in the Newtonian manner above quoted, eſpecially in a well darkened room, may remain bright enough to ſerve the purpoſe of microſcopic illumination, in which caſe more preciſion can eaſily be obtained.

The greateſt cauſe for a mixture of colours, however, which is the breadth of the priſm, I ſaw might eaſily be removed; therefore, on account of the coloured points, which have been mentioned in the 6th and 7th experiments, I was willing to try whether they proceeded from this mixture; and therefore covered the priſm in front with a piece of paſteboard, having a ſlit in it of about one-tenth of an inch broad.

10th Experiment. The nail at 9 feet 2 inches from the priſm.—*R.* I fix my attention on two ſhining red points; they are pretty bright.—*O.* I ſee many more points; the object is better illuminated than in the red; the points are ſurrounded by black, but are orange-coloured.—*Y.* The points now are yellow, and white ſurrounded by black; the object is better illuminated than in orange. The maximum of illumination is in the brighteſt yellow or paleſt green.—*G.* The points are green and white, as before ſurrounded by black; better illuminated than in orange.—*B.* The illumination is nearly equal to red.—*I.* Very indifferently illuminated.—*V.* Very badly illuminated.

The phænomena of the differently coloured points being now completely reſolved, ſince they were plainly owing to the former admixture of colours, and the illuminating power remaining aſcertained as before, I attempted alſo to repeat the experiments upon the thermometer, with the priſm covered in the ſame manner; but I found the effect of the coloured rays too much enfeebled to give a deciſive reſult.

I might now proceed to my next ſubject; but it may be pardonable if I digreſs for a moment, and remark that the foregoing reſearches ought to lead us on to others. May not the chemical properties of the priſmatic colours be as different as thoſe which relate to light and heat? Adequate methods for an inveſtigation of them may eaſily be found; and we cannot too minutely enter into an analyſis of light, which is the moſt ſubtile of all the active principles that are concerned in the mechaniſm of the operations of nature. A better acquaintance with it may enable us to account for various facts that fall under our daily obſervation, but which have hitherto remained unexplained. If the power of heating, as we now ſee, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the ſtate of a red glow; and for the heat which is given by charcoal, coke, and balls of ſmall-coal mixed up with clay, uſed in hot-houſes; all which, it is well known, throw out red light. It alſo explains the reaſon why the yellow, green, blue, and purple flames of burning ſpirits mixed with ſalt, occaſion ſo little heat, that a hand is not materially injured when paſſed through their coruſcations. If the chemical properties of colours alſo, when aſcertained, ſhould be ſuch that an acid principle, for inſtance, which has been aſcribed to light in general, on account of its changing the complexion of various ſubſtances expoſed to it, may reſide only in one of the colours, while others may prove to be differently inveſted, it will follow that bodies may be variouſly affected by light, according as they imbibe and retain, or tranſmit and reflect, the different colours of which it is compoſed.

Radiant Heat is of different Refrangibility.

I muſt now remark that my foregoing experiments aſcertain, beyond a doubt, that radiant heat, as well as light, whether they be the ſame or different agents, is not only refrangible, but is alſo ſubject to the laws of the diſperſion ariſing from its different refrangibility; and, as this ſubject is new, I may be permitted to dwell a few moments upon it. The priſm refracts radiant heat, ſo as to ſeparate that which is leſs efficacious from that which is more ſo. The whole quantity of radiant heat contained in a ſun-beam, if this different refrangibility did not exiſt, muſt inevitably fall uniformly on a ſpace equal to the area of the priſm; and, if radiant heat were not refrangible at all, it would fall upon an equal ſpace in the place where the ſhadow of the priſm, when covered, may be ſeen. But neither of theſe events taking place, it is evident that radiant heat is ſubject to the laws of refraction, and alſo to thoſe of the different refrangibility of light. May not this lead us to ſurmiſe that radiant heat conſiſts of particles of light of a certain range of momenta, and which range may extend a little further, on each ſide of refrangibility, than that of light? We have ſhown that, in a gradual expoſure of the thermometer to the rays of the priſmatic ſpectrum, beginning from the violet, we come to the maximum of light long before we come to that of heat, which lies at the other extreme. By ſeveral experiments, which time will not allow me now to report, it appears that the maximum of illumination has little more than half the heat of the full red rays; and, from other experiments, I likewiſe conclude that the full red falls ſtill ſhort of the maximum of heat; which perhaps lies even a little beyond viſible refraction. In this caſe, radiant heat will at leaſt partly, if not chiefly, conſiſt, if I may be permitted the expreſſion, of inviſible light; that is to ſay, of rays coming from the ſun, that have ſuch a momentum as to be unfit for viſion. And admitting, as is highly probable, that the organs of ſight are only adapted to receive impreſſions from particles of a certain momentum, it explains why the maximum of illumination ſhould be in the middle of the refrangible rays; as thoſe which have greater or leſs momenta are likely to become equally unfit for impreſſions of ſight. Whereas in radiant heat, there may be no ſuch limitation to the momentum of its particles. From the powerful effects of a burning lens, however, we gather the information, that the momentum of terreſtrial radiant heat is not likely to exceed that of the ſun; and that, conſequently, the refrangibility of *calorific* rays cannot extend much beyond that of *colourific* light. Hence we may alſo infer that the inviſible heat of red-hot iron, gradually cooled till it ceaſes to ſhine, has the momentum of the inviſible rays which, in the ſolar ſpectrum viewed by day-light, go to the confines of red; and this will afford an eaſy ſolution of the reflection of inviſible heat by concave mirrors.

Application of the Reſult of the foregoing Obſervations to the Method of viewing the Sun advantageouſly, with Teleſcopes of large Apertures and high Magnifying Powers.

Some time before the late tranſit of Mercury over the diſk of the ſun, I prepared my 7-feet teleſcope, in order to ſee it to the beſt advantage. As I wiſhed to keep the whole aperture of the miror open, I ſoon cracked every one of the darkened ſlips of wedged glaſſes, which are generally uſed with achromatic teleſcopes: none of them could withſtand the accumulated heat in the focus of pencils, where theſe glaſſes are generally placed. Being thus left without reſource, I made uſe of red glaſſes; but was by no means ſatisfied with their performance. My not being better prepared, as it happened, was of no conſequence; the weather proving totally unfavourable for viewing the ſun at the time of the tranſit. However, as I was fully aware of the neceſſity of providing an apparatus for this purpoſe, ſince no method that was in uſe could be applied to my teleſcopes, I took the firſt opportunity of beginning my trials.

The inſtrument I wiſhed to adapt for ſolar inſpection, was a Newtonian reflector, with 9 inches aperture; and my aim was to uſe the whole of it open.

I began with a red glaſs; and, not finding it to ſtop light enough, took two of them together. Theſe intercepted full as much light as was neceſſary; but I ſoon found that the eye could not bear the irritation from a ſenſation of heat, which it appeared theſe glaſſes did not ſtop.

I now took two green glaſſes: but found that they did not intercept light enough. I therefore ſmoked one of them; and it appeared that, notwithſtanding they now ſtill tranſmitted conſiderably more light than the red glaſſes, they remedied the former inconvenience of an irritation ariſing from heat. Repeating theſe trials ſeveral times, I conſtantly found the ſame reſult; and, the ſun in the firſt caſe being of a deep red colour, I ſurmiſed that the red-making rays, tranſmitted through red glaſſes, were more efficacious in raiſing a ſenſation of heat than thoſe which paſſed through green, and which cauſed the ſun to look greeniſh. In conſequence of this ſurmiſe, I undertook the inveſtigations which have been delivered under the two firſt heads.

As ſoon as I was convinced that the red light of the ſun ought to be intercepted on account of the heat it occaſions, and that it might alſo be ſafely ſet aſide, ſince it was now proved that pale green light excels in illumination, the method which ought to be purſued in the conſtruction of a darkening apparatus was ſufficiently pointed out; and nothing remained but to find ſuch materials as would give us the colour of the ſun, viewed in a teleſcope, of a pale green light, ſufficiently tempered for the eye to bear its luſtre.

To determine what glaſſes would moſt effectually ſtop the red rays, I procured ſome of all colours, and tried them in the following manner:—I placed a priſm in the upper part of a window, and received its coloured ſpectrum upon a ſheet of white paper. Then I intercepted the colours, juſt before they came to the paper, ſucceſſively, by the glaſſes, and found the reſult as follows: a deep red glaſs intercepted all the rays; a paler red did the ſame.

From this we ought not to conclude, that red glaſſes will ſtop the red rays; but rather, that none of the ſun's light, after its diſperſion by the priſm, remains intenſe enough to paſs through red glaſſes in ſufficient quantity to be perceptible when it comes to the paper. By looking through them directly at the ſun, or even at day objects, it is ſufficiently evident that they tranſmit chiefly red rays.

An orange glaſs tranſmitted nearly all the red, the orange, and the yellow. It intercepted ſome of the green, much of the blue, and very little of the indigo and violet.—A yellow glaſs intercepted hardly any light of any one of the colours.—A dark green glaſs intercepted nearly all the red, and partly alſo the orange and yellow. It tranſmitted the green, intercepted much of the blue, but none of the indigo and violet.—A darker green glaſs intercepted nearly all the red, much of the orange, and a little of the yellow. It tranſmitted the green, ſtopped ſome of the blue, but tranſmitted the indigo and violet.—A blue glaſs intercepted much of the red and orange, ſome of the yellow, hardly any of the green, none of the blue, indigo, or violet.—A purple glaſs tranſmitted ſome of the red; a very little of the orange and yellow. It alſo tranſmitted a little of the green and blue, but more of the indigo and violet.

From theſe experiments we ſee that dark green glaſſes are moſt efficacious for intercepting red light, and will therefore anſwer one of the intended purpoſes; but ſince in viewing the ſun we have alſo its ſplendour to contend with, I proceeded to the following additional trials:

White glaſs, lightly ſmoked, apparently intercepted an equal ſhare of all the colours; and when the ſmoke was laid on thicker, it permitted none of them to paſs. Hard pitch, melted between two white glaſſes, intercepted much light; and when put on ſufficiently thick, tranſmitted none. Many differently coloured fluids that were alſo tried, I found were not ſufficiently pure to be uſed, when denſe enough to ſtop light. Now red glaſſes, and the two laſt-mentioned reſources of ſmoke and pitch, any one of which, it has been ſeen, will ſtop as much light as may be required, had ſtill a remaining trial to undergo, relating to diſtinctneſs; but this I was convinced could only be decided by actual obſervations of the ſun. As an eaſy way of ſmoking glaſſes uniformly is of ſome conſequence to diſtinct viſion, it may be of ſervice here to give the proper directions how to proceed in the operation:—With a pair of warm pliers, take hold of the glaſs, and place it over a candle at a ſufficient diſtance not to contract ſmoke. When it is heated, but no more than ſtill to permit a finger to touch the edges of it, bring down the glaſs at the ſide of the flame, as low as the wick will permit, which muſt not be touched; then, with a quick vibratory motion, agitate it in the flame from ſide to ſide; at the ſame time advancing and retiring it gently all the while. By this method you may proceed to lay on ſmoke to any required darkneſs. It ought to be viewed from time to time, not only to ſee whether it be ſufficiently dark, but whether any inequality may be perceived; for if that ſhould happen, it will not be proper to go on.

The ſmoke of ſealing-wax is bad; that of pitch is worſe. A wax candle gives a good ſmoke; but that of a tallow candle is better. As good as any I have hitherto met with is the ſmoke of ſpermaceti oil. In uſing a lamp, you may alſo have the advantage of an even flame extended to any length.

Teleſcopic Experiments.

No. 1. By way of putting my theory to the trial, I uſed two red glaſſes, and found that the heat which paſſed through them could not be ſuffered a moment; but I was now alſo convinced that diſtinctneſs of viſion is capitally injured by the colouring matter of theſe glaſſes.

No. 2. I ſmoked a white glaſs till it ſtopped light enough to permit the eye to bear the ſun. This deſtroyed all diſtinctneſs;

neſs; and alſo permitted ſome heat to come to the eye by tranſmitting chiefly red rays.

No. 3. I applied two white glaſſes, with pitch between them, to the teleſcope; and found that it made the ſun appear of a ſcarlet colour. They tranſmitted ſome heat; and diſtinctneſs was greatly injured.

No. 4. I uſed a very dark green glaſs to ſtop heat; and behind it, or towards the eye, I placed a red glaſs to ſtop light. The firſt glimpſe I had of the ſun was accompanied with a ſenſation of heat; diſtinctneſs alſo was materially injured.

No. 5. I uſed a dark green and a pale red; but the ſun not being ſufficiently darkened, I ſmoked the red glaſs, and putting a ſmall partition between the two, placed the ſmoke towards the green glaſs. This took off the exuberance of light; but did not remedy the inconvenience ariſing from heat.

No. 6. I uſed two pale green glaſſes, ſmoking that next to the eye, and placing it as in No. 5, ſo that the ſmoke might be incloſed between the two. This acted incomparably well; but in a very ſhort time, the heat which paſſed the firſt glaſs (though not the ſecond, for I felt no ſenſation of it in the eye,) diſordered the ſmoke, by drawing it up into little bliſters or ſtars, which let through light; and this compoſition therefore ſoon became uſeleſs.

No. 7. I uſed two dark green glaſſes, one of them ſmoked, as in No. 5. Theſe alſo acted well; but became uſeleſs for the reaſon aſſigned in No. 6, though ſomewhat leſs ſmoke had been required than in the former compoſition. I felt no heat.

No. 8. I uſed one pale green, with a dark green ſmoked glaſs upon it, as in No. 5. It bore an aperture of four inches very well, and the ſmoke was not diſordered; but when all the tube was open, the pale green glaſs cracked in a few minutes.

No. 9. Placing now a dark green before a ſmoked green, I ſaw the ſun remarkably well. In this experiment I had made a difference in the arrangement of the apparatus. The cracking of the glaſſes, I ſuppoſed, might be owing to their receiving heat in the middle, while the outſide remained cold; which would occaſion a partial dilatation. I therefore cut them into pieces about a quarter of an inch ſquare, and ſet three of them in a ſlider, ſo that I could move them behind the ſmoked glaſs without diſturbing it. After looking about three or four minutes through one of them, I moved the ſlider to the ſecond, and then to the third. This kept the glaſſes ſufficiently cool: but the diſturbance of the alterations proved hurtful to viſion, which requires repoſe; and, if perchance I ſtopped a little longer than the proper time, the glaſs cracked with a very diſagreeable exploſion that endangered the eye.

No. 10. Two dark green glaſſes, both ſmoked, that a thinner coat might be on each; but the ſmoke ſtill contracted bliſters, though leſs denſe than before.

No. 11. To get rid of ſmoke entirely, I uſed two dark green glaſſes, two very dark green, two pale blue, and one pale green glaſs, together. Diſtinctneſs was wanting, nor was light ſufficiently intercepted.

No. 12. A dark green and a pale blue glaſs ſmoked. The green glaſs cracked.

No. 13. A pale blue and a dark green glaſs ſmoked. The blue glaſs cracked. The eye felt no ſenſation of heat.

No. 14. Two pale blue glaſſes, one ſmoked. The firſt glaſs cracked.

It was now ſufficiently evident that no glaſs which ſtops heat, and therefore receives it, could be preſerved from cracking, when expoſed to the focus of pencils. This induced me to try an application of the darkening apparatus to another part of the teleſcope.

The place where the rays are leaſt condenſed, without interfering with the reflections of the mirrors, is immediately cloſe to the ſmall one. I therefore ſcrewed an apparatus to the ſpeculum arm, into which any glaſs might be placed.

No. 15. A dark green glaſs cloſe to the ſmall ſpeculum, and ſmoked pale green in the focus of pencils as before. I ſaw remarkably well.

No. 16. The dark green as before; but, that more light might be admitted, a white ſmoked glaſs near the eye. Better than No. 15; but the green glaſs cracked.

No. 17. A very dark green and white ſmoked glaſs, as before. Very diſtinct, but the green glaſs cracked in about ſix or ſeven minutes.

No. 18. A dark blue glaſs, as in No. 15, and white ſmoked. This was diſtinct, and no heat came to the eye. The ſun appeared ruddy.

No. 19. A dark blue and a yellow glaſs, cloſe together, as in No. 15, and a white ſmoked one as before. This was not diſtinct, [illegible]

No. 20. A purple glaſs, as in No. 15, with a white ſmoked one. This gave the ſun of a deep orange colour, approaching to ſcarlet. It was not diſtinct.

No. 21. An orange glaſs, as in No. 15, with a white ſmoked one. The colour of the ſun was too red.

No. 22. A white ſmoked glaſs, as in No. 15, without any other at the eye. This gave the ſun of a beautiful orange colour; but diſtinctneſs was totally deſtroyed.

No. 23. The heat near the ſmall ſpeculum being ſtill too powerful for the glaſſes, I had a blueiſh dark green glaſs made of a proper diameter to be incloſed between the two eye-glaſſes of a double eye-piece. All glaſs I knew would ſtop ſome heat; and was therefore in hopes that the interpoſition of this eye-glaſs would temper the rays, ſo as in ſome meaſure to protect the coloured glaſs. In the uſual place near the eye, I put two white glaſſes, with a thin coat of pitch between them. Theſe glaſſes, when looked through by the natural eye, give the ſun of a red colour; I therefore entertained no great hopes of their application to the teleſcope. They darkened the ſun not ſufficiently; and when the pitch was thickened diſtinctneſs was wanting.

No. 24. The ſame glaſs between the eye-glaſſes, and a dark green ſmoked glaſs at the eye; very diſtinct. This arrangement is preferable to that of No. 15; after ſome conſiderable time, however, this glaſs alſo cracked.

No. 25. I placed a very dark green glaſs behind the ſecond eye-glaſs, that it might be ſheltered by both glaſſes, which in my double eye-piece are cloſe together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glaſs receives them on a large ſurface, and ſtops light and heat in the proportion of the ſquares of its diameter now uſed, to that on which the rays would have fallen had it been placed in the focus of pencils; and for the ſame reaſon I now alſo placed a dark green ſmoked glaſs cloſe upon the former, with the ſmoked ſide towards the eye, that the ſmoke might likewiſe be protected againſt heat by a paſſage of the rays through two ſurfaces of coloured glaſs.

This poſition had moreover the advantage of leaving the teleſcope, with its mirrors and glaſſes, completely to perform its operation before the application of the darkening apparatus; and thus to prevent the injury which muſt be occaſioned by the interpoſition of the heterogeneous colouring matter of the glaſſes and of the ſmoke.

No. 26. I placed a deep blue glaſs with a blueiſh green ſmoked one upon it, as in No. 25, and found the ſun of a whiter colour than with the former compoſition. There was no diſagreeable ſenſation of heat, though a little warmth might be felt.

No. 27. I uſed two black glaſſes, placed as in No. 25. Here there was no occaſion for ſmoke; but the ſun appeared of a bright ſcarlet colour, and an intolerable ſenſation of heat took place immediately. I rather ſuſpect that theſe are very deep red glaſſes, though their outward appearance is black.

In order to have a more ſure criterion of heat, I applied Dr. Wilſon's thermometer, No. 2, to the end of the eye-piece, where the eye is generally placed. With No. 25, it roſe from 34 to 37 degrees; with No. 26, it roſe from 35 to 46; and with No. 27, it roſe very quickly from 36 to 95 degrees. I am pretty ſure it would have mounted up ſtill higher; but, the ſcale extending only to 100, I was not willing to run the riſk of breaking the thermometer by a longer expoſure.

It remains now only to be added, that with No. 25 and 26 I have ſeen uncommonly well; and that, in a long ſeries of very intereſting obſervations upon the ſun, which will

ſoon

will ſoon be communicated, the glaſſes have met with no accident. However, when the ſun is at a conſiderable altitude, it will be adviſable to leſſen the aperture a little, in teleſcopes that have ſo much light as my 10-feet reflector; or, which will give us more diſtinctneſs, to view the ſun earlier in the morning and later in the afternoon; for the light, intercepted by the atmoſphere in lower altitudes, will reduce its brilliancy much more uniformly than we can ſoften it by laying on more ſmoke upon our darkening glaſſes. Now, as few inſtruments in common uſe are ſo large as that to which this method of darkening has been adapted, we may hope that it will be of general utility in ſolar obſervations.

Slough, near Windſor,
March 3, 1806.

III. *Account of a Series of Experiments, undertaken with the View of decompoſing the Muriatic Acid.* By Mr. WILLIAM HENRY.

[Concluded from Page 218.]

A GREAT variety of ſimilar experiments convinced me, that, by electrifying together the carbonated hydrogenous and muriatic gaſes, not the ſmalleſt progreſs was made towards the decompoſition of the latter. All that was thus effected conſiſted in the decompoſition of the water of the two gaſes by the carbon of the combuſtible gas; and, when this was completely accompliſhed, no further effect enſued from continuing the electriſation. The generation of carbonic acid was proved by the following experiment:

Exper. 14. To a mixture of carbonated hydrogen and muriatic gaſes, after having received above 100 ſhocks, a drop of water was admitted, which abſorbed the muriatic acid. The liquid was then taken up by blotting-paper; and the reſiduary gas, being transferred into another tube, was brought into contact with a ſolution of pure barytic earth. The precipitation of this ſolution evinced the preſence of carbonic acid.

It was deſirable, however, that the effects ſhould be aſcertained;

tained, of electrifying together pure muriatic acid and pure carbonated hydrogenous gas, both perfectly free from water. Now, from the experiments related in the firſt ſection, it appears highly probable that a complete purification from moiſture is produced, in both gaſes, by the action of the electric fluid; all the water they before contained being thus decompoſed. In the following experiments, therefore, the two gaſes were ſeparately electrified before they were ſubmitted to this proceſs conjointly.

Exper. 15. To a portion of muriatic acid, diminiſhed by the action of electricity from 144 to 121 meaſures, 27 meaſures of carbonated hydrogenous gas, expanded as far as poſſible, were added, and 200 ſhocks paſſed through the mixture. The addition of permanent gas amounted to 14 meaſures; 10 of which may be traced to the muriatic acid, and were evolved by its ſeparate electrifation. The remaining four meaſures, which remain to be accounted for, are too ſmall a quantity to be aſcribed to the decompoſition of the acid.

Exper. 16. To a quantity of carbonated hydrogenous gas, which had received 400 ſhocks, and occupied the ſpace of 212 meaſures, I added 232 of muriatic acid, through which 200 ſhocks had previouſly been paſſed. The electrifation of the mixture was next continued till 800 diſcharges had taken place. On examining the mixture of gaſes during this operation, no change whatever took place; and, after its cloſe, no more muriatic acid had diſappeared than would have been deficient after the firſt electrifation; nor was there any further production of permanent gas.

Exper. 17. The ſame reſult was obtained by electrifying together 280 meaſures of carbonated hydrogenous gas, previouſly expanded by 600 ſhocks, and 114 of muriatic acid, after 400 ſhocks. The additional diſcharge, through this mixture, of 1000 ſhocks, did not evince the ſmalleſt progreſs towards the decompoſition of the muriatic acid.

Exper. 18. In the naturally moiſt ſtate of theſe gaſes, it follows, from the 14th experiment, that carbonic acid is produced by electrifying them in conjunction. It appeared to me of ſome importance to aſcertain whether, after a previous decompoſition of their moiſture, carbonic acid would continue to be generated. But the electrified carbonated hydrogenous gas itſelf contains carbonic acid, which, unleſs removed, would render the reſult of the experiment undecitive. This was accompliſhed by paſſing up, to a portion of electrified gas, a bubble or two of dry ammoniacal gas; which, uniting with the carbonic acid, would condenſe any portion of it that might be preſent. The remainder was transferred into another tube; and to this carbonated hydrogenous gas, perfectly deprived both of moiſture and carbonic acid, muriatic acid gas, previouſly electrified, was added, and electrical ſhocks were paſſed through the mixture. A drop of water was then admitted; and the reſiduary gas, after having been dried, was transferred into another tube. On paſſing up barytic water, not the ſmalleſt trace of carbonic acid could be diſcovered.

From the preceding experiments, the following concluſions may be deduced:

1. The muriatic acid gas, in the drieſt ſtate in which it can be procured, ſtill contains a portion of water. From a calculation founded on the experiments deſcribed in the firſt ſection, the grounds of which are too obvious to require being ſtated, it follows that 100 cubical inches of muriatic gas, after expoſure to muriat of lime, ſtill hold in combination 1.4 grain of water.

2. When electrical ſhocks are paſſed through this gas, the watery portion is decompoſed. The hydrogen of the water, uniting with the electric matter, conſtitutes hydrogenous gas, and the oxygen unites with the muriatic acid; which laſt, acting on the mercury, compoſes muriat of mercury.

3. The electric fluid ſerves as an intermedium in combining oxygen with muriatic acid.

4. The really acid portion of muriatic gas does not ſuſtain any decompoſition by the action of electricity.

5. When electric ſhocks are paſſed through a mixture of carbonated hydrogen and muriatic acid gaſes, the water held in ſolution by theſe gaſes is decompoſed by the carbon of the compound inflammable gas; and carbonic acid and hydrogenous gaſes are the reſult.

6. When all the water of the two gaſes has been decompoſed, no effect enſues from continuing the electriſation; or, if the water of each gas has been previouſly deſtroyed by electrifying them ſeparately, no further effect enſues from electrifying them conjointly.

7. Since, therefore, carbon, though placed under the moſt favourable circumſtances for abſtracting from the muriatic acid, and combining with its oxygen, evinces no ſuch tendency, it may be inferred, that, if the muriatic acid be an oxygenated ſubſtance, its radical has a ſtronger affinity for oxygen than charcoal poſſeſſes.

Though the firſt impreſſions excited in my mind by the total failure of the above experiments, in accompliſhing one of the greateſt objects of modern chemiſtry, have induced me for ſome time to withhold them from the ſociety, I am ſatisfied, by reflection, that this communication is not without expediency. The means employed in attempting the analyſis of the muriatic acid, were ſuch as, after mature deliberation, appeared to me moſt to promiſe ſucceſs; and the experiments were attended with a degree of labour, which can only be eſtimated by thoſe who have been engaged in ſimilar purſuits; not one third of thoſe which were really made having been deſcribed in the foregoing account of them. It may ſpare, therefore, to others, a fruitleſs application of time and trouble, to be made acquainted with what I have done; and the collateral facts, which have preſented themſelves in the inquiry, are perhaps not without curioſity or value.

From the reſult of theſe experiments, I apprehend, all hope muſt be relinquiſhed of effecting the decompoſition of the muriatic acid in the way of ſingle elective affinity. They furniſh alſo a ſtrong probability that the baſis of the muriatic acid is ſome unknown body; for no combuſtible ſubſtance with which we are acquainted can retain oxygen, when ſubmitted in contact with charcoal, to the action of electricity, or of a high temperature. The analyſis of this acid muſt, in future, be attempted with the aid of complicated affinities. Thus, in the maſterly experiment of Mr. Tennant, phoſphorus, which attracts oxygen leſs ſtrongly than charcoal, by the intermediation of lime, decompoſes the carbonic acid.

Yet,

Yet, led by the analogy of this fact, its diſcoverer found that a ſimilar artifice did not ſucceed in decompoſing the muriatic acid. "As vital air," he obſerves, "is attracted by a compound of phoſphorus and calcareous earth more powerfully than by charcoal, I was deſirous of trying their efficacy upon thoſe acids which may from analogy be ſuppoſed to contain vital air, but which are not affected by the application of charcoal. With this intention, I made phoſphorus paſs through a compound of marine acid and calcareous earth, and alſo of fluor acid and calcareous earth, but without producing in either of them any alteration. Since the ſtrong attraction which theſe acids have for calcareous earth tends to prevent their decompoſition, it might be thought, that in this manner they were not more diſpoſed to part with vital air than by the attraction of charcoal: but this, however, does not appear to be the fact. I have found that phoſphorus cannot be obtained by paſſing marine acid through a compound of bones and charcoal when red-hot. The attraction, therefore, of phoſphorus and lime for vital air exceeds the attraction of charcoal by a greater force than that ariſing from the attraction of marine acid for lime *."

By means ſimilar to thoſe employed in attempting the analyſis of the muriatic acid, I tried to effect that of the fluoric acid. When electrified alone, in a glaſs tube coated internally with wax, it ſuſtained a diminution of bulk, and there remained a portion of hydrogenous gas. But neither in this mode, nor by ſubmitting it, mixed with carbonated hydrogenous gas, to the action of electricity, was any progreſs made towards its analyſis. Theſe experiments, however, render it probable, that the fluoric acid, like the muriatic, is ſuſceptible of ſtill further oxygenation, in which ſtate it becomes capable of acting on mercury. The carbonic acid, on the contrary, appears not to admit of two different degrees of oxygenation. When the electric ſhock has been repeatedly paſſed through a portion of this acid gas, its bulk is enlarged, and a permanent gas is produced, which is evidently a mixture of oxygenous and hydrogenous gaſes; for, when

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* Philoſophical Tranſactions, Vol. LXXI. p. 184.

an electric ſpark is paſſed through the gas that remains after the abſorption of the carbonic acid by cauſtic alkali, it immediately explodes. Theſe reſults even take place on electrifying carbonic acid from marble, previouſly calcined in a low red heat, to expel its water, and then diſtilled in an earthen retort*.

IV. *Experiments in Galvanic Electricity, by Meſſrs.* NICHOLSON, CARLISLE, CRUICKSHANK, &c.

IN the fourth volume of the Philoſophical Magazine, p. 59, 163, and 306, we laid before our reader's M. Volta's account of the progreſs he had then made in Galvaniſm; and in our number for May laſt we ſtated that Mr. Carliſle had been making ſome experiments which were likely to lead to ſome important diſcovery in Galvanic electricity. We ſhould have then mentioned, but we knew not the fact, that he made them in conjunction with Mr. Nicholſon, editor of a well conducted and well known philoſophical journal. When we learnt the circumſtance, motives of delicacy, which our readers muſt approve, forbade our attempting to give any further particulars till Mr. Nicholſon himſelf, who had the beſt right, ſhould firſt lay them before the public.

Since that time Mr. Nicholſon has publiſhed ſeveral curious and important papers on this ſubject, from which we ſhall now ſelect a few particulars for the information of our readers; referring thoſe who may wiſh for a fuller account, to Mr. Nicholſon's journal.

The general apparatus we need not particularly to deſcribe, having in the preſent number given a tranſlation of M. Volta's paper which appeared in the ſecond part of the Philoſophical Tranſactions for the preſent year, juſt publiſhed.

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* Meſſieurs Landriani and Van Marum (*An. de Chemie*, Tome II. p. 270.) obtained only hydrogenous gas by electrifying the carbonic acid gas. But the conductor of their apparatus was an iron one; which metal would combine with the oxygen of the water, and prevent it from appearing in a gaſeous ſtate. In my experiments the conductors were of platina.

Mr. Anthony Carliſle, having been favoured with a peruſal of this paper by the very reſpectable Preſident of the Royal Society, the Right Hon. Sir Joſeph Banks, Bart. ſoon after, in conjunction with Mr. Nicholſon, began to repeat the experiments of M. Volta, and obtained ſimilar reſults. Very early in this courſe, the contacts being made ſure by placing a drop of water upon the upper plate of the pile, Mr. Carliſle obſerved a diſengagement of gas round the conducting wire. This gas, though minute in quantity, ſeemed to Mr. Nicholſon to have the ſmell of hydrogen when the wire of communication was ſteel. This, with ſome other facts, led him to propoſe to break the circuit by the ſubſtitution of a tube of water between two wires. Accordingly, a braſs wire through each of two corks was inſerted at the oppoſite ends of a glaſs tube about half an inch in diameter, filled between the corks with water: the diſtance between the points of the wires in the water, was about an inch and three quarters.

"This compound diſcharger was applied ſo that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-ſix half crowns with the correſpondent pieces of zinc and paſteboard. A fine ſtream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the ſilver, and the oppoſite point of the upper wire became tarniſhed, firſt deep orange, and then black. On reverſing the tube, the gas came from the other point, which was now loweſt, while the upper, in its turn, became tarniſhed and black. Reverſing the tube again, the phenomena again changed their order. In this ſtate the whole was left for two hours and a half. The upper wire gradually emitted whitiſh filmy clouds, which, towards the end of the proceſs, became of a pea-green colour, and hung in perpendicular threads from the extreme half inch of the wire, the water being rendered ſemi-opaque by what fell off, and in a great part lay, of a pale green, on the lower ſurface of the tube, which, in this diſpoſition of the apparatus, was inclined about forty degrees to the horizon. The lower wire, three quarters of an inch long, conſtantly emitted gas, except when another circuit, or complete wire, was applied to the apparatus; during which time the emiſſion of gas was ſuſpended. When this laſt mentioned wire was removed, the gas reappeared as before, not inſtantly, but after the lapſe of four beats of a half ſecond clock ſtanding in the room. The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread."

To have reverſed the tube would have anſwered the ſame purpoſe, but they choſe to do this, "and found that, when the zinc was at the bottom, its effects were reverſed; that is to ſay, the gas ſtill came from the wire communicating with the ſilver," &c.

Meſſrs. Carliſle and Nicholſon were "led, by reaſoning on the firſt appearance of hydrogen, to expect a decompoſition of water; but it was with no little ſurpriſe that they found the hydrogen extricated at the contact with one wire, while the oxygen fixed itſelf in combination with the other wire at the diſtance of almoſt two inches. As the diſtance between the wires formed a ſtriking feature in this reſult, it became deſirable to aſcertain whether it would take place to greater diſtances. When a tube three quarters of an inch in diameter, and thirty-ſix inches long, was made uſe of, the effect failed, though the very ſame wires, inſerted into a ſhorter tube, operated very briſkly."

The experiment being tried with tincture of litmus in place of water, and the oxydating wire, namely, from the zinc ſide, being loweſt in the tube, it changed the tincture red in about ten minutes as high as the upper extremity of the wire. The other portion remained blue. Hence it ſeems either an acid was formed, or that a portion of the oxygen combined with the litmus, ſo as to produce the effect of an acid.

"It may be here offered as a general remark, that the electric pile with card, or with woollen cloth, continues in order for about two days, or ſcarcely three; that, from a ſeries of glaſſes ſet up by Mr. Carliſle, as well as from the pile itſelf, it appears that the ſame proceſs of decompoſition of water is carried on between each pair of plates, the zinc being oxyded on the wet face, and hydrogen given out; that the common ſalt is decompoſed, and exhibits an efflorescence of ſoda round the edges of the pile, extruded, moſt probably, by the hydrogen: and that, on account of the corroſion of the faces of the zinc, it is neceſſary to renew them previouſly to each conſtruction of the pile. This may be done by ſcraping or grinding."

By ſeveral accurate and well conducted experiments, Mr. Nicholſon aſcertained that the electricity of the zinc was plus, and that of the ſilver minus, which ever of them were at the top of the pile. The electric ſpark was even rendered viſible; ſo that there can be no doubt of the identity of the electric and Galvanic fluid.

The decompoſition of water, and oxydation of metallic wire, ſuggeſted other experiments. Two ſmall wires of platina were inſerted, as before, in a ſhort tube. When the connection with the pile was formed, the wire from the ſilver gave a plentiful ſtream of gas, and that from the zinc a ſmaller one. In four hours, neither turbidneſs, oxydation, nor tarniſh appeared. The larger ſtream was naturally ſuppoſed to be hydrogen, the ſmaller oxygen.

With thick gold leaf inſtead of platina, the reſult was the ſame, only the extremity of the ſlip connected with the zinc acquired a coppery or purpliſh tinge.

A braſs wire was ſubſtituted for one of the ſlips of gold. When the former was joined to the ſilver end, the two ſtreams were extricated as before; but when joined to the zinc, it became oxyded, as when both the wires were of braſs.

The ſimple decompoſition of water by platina wires, without oxydation, offered a means of obtaining the gaſes ſeparate from each other. This was tried with a pile of ſixty-eight ſets. A wire from each end of the pile paſſed under ſeparate phials full of water inverted in a ſaucer of water. "A cloud of gas aroſe from each wire, but moſt from the ſilver or minus ſide. Bubbles were extricated from all parts of the water, and adhered to the whole internal ſurface of the veſſels. The proceſs was continued for thirteen hours, after which the wires were diſengaged, and the gaſes decanted into ſeparate bottles. On meaſuring the quantities, which was done by weighing the bottles, it was found that the quantities of water diſplaced by the gaſes were, reſpectively, 72 grains by the gas from the zinc ſide, and 142 grains by the gas from the ſilver ſide; ſo that the whole volume of gas was 1.17 cubic inches, or near an inch and a quarter. Theſe are nearly the proportions in bulk of what are ſtated to be the component parts of water. The gas from the zinc ſide being tried with one meaſure of nitrous gas, contracted to 1.25, and did not contract more by the addition of another meaſure; the gas from the ſilver ſide, by the ſame treatment, contracted to 1.6. The air of the room, on trial, contracted to 1.28. From the ſmallneſs of the quantity, no attempt was made to detonate the air from the zinc ſide; but a portion of that from the ſilver ſide, being mixed with one-third of atmoſpheric air, gave a loud detonation.

" Upon the above it may be remarked, that it does not ſeem probable that oxygen was afforded by both wires, but that they were mixed by the circumſtances of the experiment."

Mr. Cruickſhank, of Woolwich, alſo made ſome intereſting experiments on this ſubject. He employed plates of zinc and ſilver about 1.6 inches ſquare; and the number of each varied from 40 to 100, according to the power required. He uſed ſilver wire both from the zinc and ſilver plate in his firſt experiments; but to diſtinguiſh the ends of the pile, that wire only is called the ſilver wire, which was connected with the ſilver plate; the other wire he calls (to ſave circumlocution) the zinc wire. Theſe wires were paſſed through corks fitted into a glaſs tube filled with water, and one of the corks made perfectly tight by means of cement. " The tube was then placed upright in a cup containing water, with the uncemented end downwards. As ſoon as the communication was made between the extremities of the pile by the wires, a quantity of ſmall air bubbles began to aſcend from the end of the wire connected with the ſilver, as obſerved by Meſſrs. Nicholſon and Carliſle; but a white cloud at the ſame time made its appearance at the one proceeding from the zinc, or the zinc wire. This cloud gradually increaſed, and aſſumed a darker colour, and at laſt it became purple, or even black. A very few air bubbles were likewiſe collected upon and aſcended from this wire; but when the machine was in full force, a conſiderable ſtream could be obſerved.

"The gas was collected, and found to be a mixture of hydrogen and oxygen, in the proportion of three parts of the former to one of the latter. No great dependence however, was placed upon this in point of accuracy. The zinc wire was found to be much corroded, and looked as if a conſiderable portion of it had been diſſolved. As the cloud which was formed around this wire became purple on expoſure to the light, Mr. Cruickſhank ſuſpected it might be luna cornea, or muriat of ſilver proceeding from the ſilver, which had been ſomehow diſſolved, and afterwards precipitated in this ſtate, by the muriatic ſalts in the common water."

Diſtilled water, to which a little tincture of litmus was added, was next employed in the tube. Gas aroſe from both wires, but in greateſt quantity from the ſilver wire. In a ſhort time, the whole fluid below the point of the zinc wire became red, and the fluid above the ſilver wire looked of a deeper blue than before, the ſlight tinge of purple being deſtroyed.

Diſtilled water, tinged with Brazil wood, ſoon became of as deep a purple as could be produced by ammonia, while the portion of the fluid round the zinc wire became very pale. From theſe experiments it appears to Mr. Cruickſhank, that an acid, probably the nitrous, is produced at the wire connected with the zinc, and an alkali, probably ammonia, at that connected with the ſilver, end of the pile.

When lime-water was employed, the wire was likewiſe acted upon, but in a leſs degree. The cloud at firſt had an olive colour, exactly reſembling the precipitate of ſilver by lime water.

In theſe experiments the quantity of ſilver diſſolved was conſiderable, and, where water was employed, a portion of it remained in ſolution, which was proved by adding muriatic acid. More would probably have been ſuſpended, but that an evident precipitation near the upper extremity of the zinc wire, was occaſioned by the alkali generated by the proceſs.

As hydrogen gas, when heated, or in its naſcent ſtate, reduces metallic oxyds, Mr. Cruickſhank filled the glaſs tube with a ſolution of acetite of lead to ſeparate the hydrogen from the oxygen, and thus obtain the latter pure. An exceſs of acid was added to the acetite to take up the alkali: in a minute or two after the communication was made, ſome fine metallic needles, which afterwards aſſumed the form of a feather, or rather that of the cryſtals of ammonia, were perceivable at the end of the ſilver wire. The lead was in its metallic ſtate.

Solutions of ſulphat of copper and nitrat of ſilver, were tried in the ſame way, and with ſimilar reſults. The metals were revived.

When pure water, mixed with diſtilled vinegar or with a very little ſulphuric acid, were employed in the tube, metallic ſilver was precipitated by the ſilver wire, the acid employed preventing the alkali from precipitating the ſilver diſſolved by the generated acid; in conſequence of which, when a ſufficient quantity of the metal was taken up, it was again thrown down by the ſilver wire in its metallic form.

Muriat of ammonia in ſolution being tried, a little gas was diſengaged from the ſilver wire: an incruſtation of luna cornea was formed round the zinc wire. The liquor remaining after the experiment, ſmelled ſtrongly of ammonia. Common ſalt was decompoſed alſo. Indeed, when a ſolution of muriat of ſoda or of ammonia is employed to moiſten the papers in the pile, the ſalt is always decompoſed.

Nitrat of magneſia was decompoſed by the ſame means.

In ſome after experiments gold wires were tried, and the quantity of oxygen gas obtained was much greater than when ſilver wires were employed.

Two gold wires were paſſed through a cork looſely introduced into the mouth of a three-ounce phial filled with lime water: the phial being inverted over pure water, the exterior ends of the wires were connected with the pile in the uſual way. In four hours the phial was filled with gas extricated from the wires, eſpecially the one connected with the ſilver. One meaſure of the gas mixed with two of nitrous gas, a diminution of one meaſure took place: the reſiduum contained nitrous gas mixed with hydrogen. Four meaſures exploded by the electric ſpark over mercury, diſappeared, except about $\frac{1}{15}$ of a meaſure, which appeared to be azot.

Two gold wires were paſſed through corks, ſecured by cement in the ends of a glaſs tube, about 10 inches long, bent into the form of the letter V; they reached to within an inch of each other, at the angle, in which there was a hole about one-tenth of an inch in diameter. The tube was then filled with diſtilled water, and the opening at the angle being ſhut with the finger to keep in the water, it was thus placed in a cup of water with the angle downwards. The extremities of the wires being then joined to thoſe of the pile, gas was diſengaged from both, but moſt from that connected with the ſilver; the gaſes were thus kept diſtinct. One meaſure of the gas from the ſilver end, mixed with one of nitrous gas, gave red fumes, a diminution of one-third of a meaſure, and a reſiduum conſiſting of nitrous and hydrogen gas. Two meaſures with one of oxygen being exploded over mercury diſappeared, except about one-fifth of a meaſure, which by the nitrous teſt appeared to be chiefly oxygen. A denſe white vapour was perceived over the mercury for ſome time after the exploſion. One meaſure of the gas from the zinc end, being mixed with two of nitrous gas, the whole nearly diſappeared: another meaſure of the latter being added, the total diminution was nearly three meaſures.

With platina wires Mr. Cruickſhank obtained almoſt ſimilar reſults. The one connected with the zinc end became tarniſhed; the ſame thing happened when gold wires were uſed.

A ſolution of cryſtallized muriat of lime, incloſed in a tube in the common manner, and gold wires being employed, the one from the ſilver gave little gas, but that from the zinc a conſiderable quantity, and the fluid ſurrounding it aſſumed a fine yellow colour, a ſolution of the gold having been effected. After a time ſome gas came from the firſt wire, but there was no precipitation of lime. When the tube was opened, the fluid ſmelled of aqua regia, or the oxy-muriatic acid. When platina wires were employed in place of gold, the ſmell of nitro-muriatic acid was ſoon obſervable, but no ſolution of the platina. When the tube was filled with a ſolution of muriat of ſoda, a nitro-muriatic acid was likewiſe produced.

Mr. Cruickſhank from theſe experiments draws the following concluſions:

"1. That hydrogen gas, mixed with a very ſmall proportion of oxygen and ammonia, is ſomehow diſengaged at the wire connected with the ſilver extremity of the machine; and that this effect is equally produced, whatever the nature of the metallic wire may be, provided the fluid operated upon be pure water.

"2. That where metallic ſolutions are employed inſtead of water, the ſame wire which ſeparates the hydrogen revives the metallic calx, and depoſits it at the extremity of the wire in its pure metallic ſtate; in this caſe no hydrogen gas is diſengaged. The wire employed for this purpoſe may be of any metal.

"3. That of the earthy ſolutions, thoſe of magneſia and argill only are decompoſed by the ſilver wire; a circumſtance which ſtrongly favours the production of ammonia.

"4. That when the wire connected with the zinc extremity of the pile conſiſts either of gold or platina, a quantity of oxygen gas, mixed with a little azot and nitrous acid, is diſengaged; and the quantity of gas thus obtained is a little better than one-third of the hydrogen gas ſeparated by the ſilver wire at the ſame time.

"5. That when the wire connected with the zinc is ſilver, or any of the imperfect metals, a ſmall portion of the oxygenous gas is likewiſe given out, but the wire itſelf is either oxydated or diſſolved, or partly oxydated and partly diſſolved: indeed, the effect in this caſe, produced upon the metal, is very ſimilar to that of the concentrated nitrous acid, where a great deal of the metal is oxydated, and but a ſmall quantity held in ſolution*.

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* The great difference in the effect produced by this influence on gold and ſilver, which have always been conſidered as equally difficult to oxydate, can only be explained on the ſuppoſition, that nitrous acid is generated; for this acid, it is well known, acts powerfully on ſilver, but has no action whatever on gold. The ſame obſervation applies to platina.

" 6. That when the gaſes, obtained by gold or platina wires, are collected together and exploded over mercury, the whole nearly diſappears and forms water, with probably a little nitrous acid, for there was always a thick white vapour perceived for ſome time after the exploſion. The reſiduary gas in this caſe appeared to be azot."

Acid ſolutions of metals having been decompoſed, Mr. Cruickſhank tried their ſolution in alkalies. Pure ammonia was added to a dilute ſolution of nitrat of ſilver, till the mixture ſmelled ſtrongly of the former. Being put into a tube in the uſual way, with ſilver wires, and the communication made, a rapid production of gas took place from the ſilver end, but hardly any from the zinc. Grayiſh flaſhes of metallic ſilver were ſeparated from the ſilver wire, and on the zinc wire a dark gray ſubſtance was depoſited, which, on afterwards endeavouring to ſcrape it off with the finger, exploded, though ſtill moiſt. The wire was corroded and full of holes. The fulminating ſilver of Berthollet had in fact been produced in this experiment.

When pure ammonia was introduced into the tube in place of the ſolution of the nitrat of ſilver, the reſult was the ſame—the ſilver wire from the zinc being corroded, &c. was taken up by the alkali, and afterwards depoſited in its metallic form by the other wire. To the firſt adhered ſome of the fulminating ſilver, and a portion was alſo depoſited from the fluid, after ſtanding ſome time.

From pure ammonia, with copper wires, a quantity of very pure metal was precipitated—from an ammoniacal ſolution of copper the ſame pure metallic precipitate. From theſe experiments Mr. Cruickſhank remarks, that it appears that the Galvanic influence might be employed with ſucceſs in the analyſis of minerals.

Pure ammonia being introduced into a bottle, and inverted over the ſame fluid, with a cork and two wires, as before deſcribed, (the wire connected with the zinc being platina and the other ſilver), a rapid decompoſition of the alkali was effected. Two ounce meaſures of gas being collected and examined, was found to conſiſt of 15 parts of hydrogen gas, 13 of azotic, and two nearly of oxygen gas.

The nitrous acid ſeemed to be little or not at all acted upon by the Galvanic influence, from which Mr. Cruickſhank infers that it is ſo perfect a conductor of the fluid as to tranſmit it like metals, without experiencing any change; and this he thinks may poſſibly be owing to the great proportion of oxygen, which enters into its compoſition, having before remarked that all fluids, containing little or no oxygen, are non-conductors, or nearly ſo.

The corroded matter generated at the wire connected with the zinc are not, Mr. Cruickſhank obſerves, pure oxydes; and it follows that an acid, as before mentioned, and probably the nitrous, is produced; for all the green oxyds of copper contain an acid of ſome kind or other, the pure oxyds of that metal being either dark red or deep brown.

Mr. Nicholſon's Journal alſo contains ſome intereſting experiments on this ſubject made by Mr. Davy, of Briſtol, which gave reſults that almoſt all admit of the ſame inferences that have been made by Meſſrs. Nicholſon, Carliſle, and Cruickſhank. He made an ingenious diverſity in the circuit. Tubes filled with diſtilled water, and furniſhed with gold wires connected with the pile, were inverted in different glaſſes, and the communication between them formed by muſcular fibre; the gaſes were given off in great quantity, and from ſeveral experiments pure oxygen and pure hydrogen were ſeparately obtained, nearly in the proportions required to form water.

Some ingenious experiments have alſo been made by Mr. Henry, of Mancheſter, and by Lieut. Col. Henry Haldane; for an account of which we muſt refer our readers to the Philoſophical Journal.

V. *Letter from* HENRY MOYES, *M. D. to* MAXWELL GARTHSHORE, *M. D. containing an Account of ſome intereſting Experiments in Galvanic Electricity. Communicated by Dr.* GARTHSHORE.

DEAR SIR, Pittenweem, Fifeſhire, Aug. 15, 1800.

HAVING once more returned to my ſummer reſidence, where ſolitude increaſes the attractions of ſcience, I ſhall now fulfil my promiſe of writing to you; and having nothing better at preſent to communicate, requeſt your acceptance of the following facts, reſults of ſome experiments, which, with the aid of Mr. Nicol, I have lately made in my leiſure hours.

1ſt, When 72 ſquare pieces of copper, 72 of zinc, and 71 of moiſtened paſteboard, each containing four ſquare inches, and not exceeding in thickneſs 1-16th of an inch, were formed into a column by being applied to each other in the Galvanic ſeries, *viz.* zinc, copper, paſteboard—zinc, copper, paſteboard, &c. &c., the column, though entirely compoſed of oxydable materials, diſcovered a ſtrong Galvanic power. When the bottom of the column was touched with one hand, and the top at the ſame time with the other, both hands being wet with brine, a ſhock was felt fully as ſtrong as that which was given by an electrical jar having 80 ſquare inches of coated ſurface. Hence it appears that ſilver, or a non-oxydable metal, is by no means eſſential to the excitation of the Galvanic power; and indeed I apprehend that a Galvanic column, conſiſting of copper, zinc, and paſteboard, will be found as powerful as an equal column conſiſting of ſilver, zinc, and paſteboard. The above column, conſiſting of copper, zinc, and paſteboard, retained its power with little diminution during a period of 36 hours.

2nd, When pieces of copper, zinc, and moiſtened alumine were formed into a column by being applied to each other in the Galvanic ſeries, *viz.* zinc, copper, alumine, &c. the column gave ſhocks remarkably ſtronger than thoſe it would have given had paſteboard been uſed. Hence a ſeries of curious experiments ſpontaneouſly preſent themſelves, demanding to be tried. Hence a probability that the Galvanic power may ſometimes occur among the ſtrata of the earth; and hence a new theory of earthquakes will in all probability ere long appear.

3rd, When the alumine in the Galvanic column was moiſtened with brine, inſtead of water, the ſurface of the column became, in the courſe of three or four days, covered with a copious white effloreſcence, which when collected and examined proved to be ſoda nearly ſaturated with carbonic acid. A like effloreſcence was found in a column, in the conſtruction of which no alumine was uſed. The column conſiſted of copper and zinc, ſtratified with paper moiſtened with brine, and furniſhed, you perceive, a pleaſing probability that the Galvanic action of the metallic ſubſtances may one day conduce to the arts of utility; it being capable of furniſhing, when duly employed, genuine ſoda from the ſalt of the ſea.

4th, When the Galvanic influence was tranſmitted, by means of two braſs wires, through an aqueous ſolution of the carbonat of potaſh, both were corroded where they touched the ſolution, and both produced or extricated gas. The wire, however, which came from the copper, was manifeſtly leſs corroded than the other, though it yielded the largeſt proportion of gas; and the phænomena were not perceptibly changed by ſubſtituting lime in place of the alkali.

Both the wires alſo gave gas when they were properly immerſed in ſome other fluids; but when inſerted in the legs of an inverted glaſs ſyphon, which had been previouſly filled with diſtilled water, the mode of their action was ſeemingly changed: one was corroded without giving gas, whilſt the other gave gas without being corroded; and theſe phænomena were not interrupted by filling the bend of the ſyphon with mercury. One of the wires was conſtantly corroded without giving gas, and the other gave gas without being corroded, whether the interval between their extremities was filled with pure water alone, or partly with water and partly with mercury. The mercury however muſt have wholly precluded the tranſmiſſion of oxygen from wire to wire. It ſeems impoſſible for oxygen, in any condition, firſt to deſcend and then to aſcend through a column of a fluid full ten thouſand times heavier than the air at the level of the ocean. Nothing, except the Galvanic influence, appears to have paſſed from wire to wire either without or within the ſyphon; and, if this be admitted, it will ſcarcely be affirmed that the corroſion of the wire was a ſimple oxydation.

The Galvanic action of various fluids, upon the whole or moſt of the oxydable metals, has lately opened a field of reſearch, which ſeems well entitled to perſiſting attention. I have already projected a ſet of experiments, which I truſt will afford it ſome degree of light. You ſhall have the reſults as ſoon as they are obtained, provided they ſhall ſeem deſerving your notice; and I ſhall now conclude with briefly ſubjoining, that when a metallic communication was formed in the dark between the ends of a powerful Galvanic column, an appearance of light reſembling a ſmall electric ſpark was diſtinctly perceived at the moment of contact: that the copper and zinc, when applied to each other, diſcover a ſtrong Galvanic power; yet plates of braſs had no ſuch power, even when piled with moiſtened alumine: that alumine appeared to be more Galvanic when moiſtened with brine, than when moiſtened with water; that paſteboard was decidedly more Galvanic when moiſtened with weak ſulphuric acid, than when moiſtened either with water or brine; and that by incloſing a powerful Galvanic column in a box, armed in the manner of a magnet, I have conſtructed a kind of Galvanic machine, which is eaſily carried from place to place, which is extremely convenient in many experiments, and which has enabled me to explain the powers of the torpedo more clearly, I believe, than has hitherto been done.

I have the honour to be, with great eſteem,
Dear Sir,
Your much obliged ſervant,
To Dr. Garthſhore. HENRY MOYES.

VI. *A Project for extending the Breed of Fine-Woolled Spaniſh Sheep, now in the Poſſeſſion of His Majeſty, into all Parts of Great Britain, where the Growth of fine Clothing Wools is found to be profitable.*

AFTER experiments had been tried for ſeveral years, by the King's command, with Spaniſh ſheep of the true Merino breed, imported from various parts of Spain, all of which concurred in proving, that the valuable wool of thoſe animals did not degenerate in any degree in this climate, and that the croſs of a Merino ram uniformly increaſed the quantity and meliorated the quality of the wool of every kind of ſhort-woolled ſheep on which it was tried, and more particularly ſo in the caſe of the South Down, Hereford, and Devonſhire breeds; His Majeſty was pleaſed to command that ſome Merino ſheep ſhould be procured from a flock, the character of which for a fine pile of wool was well eſtabliſhed.

Application was accordingly made to Lord Auckland, who had lately returned from an embaſſy to Spain; and in conſequence of his Lordſhip's letters, the Marchioneſs del Campo di Alange was induced to preſent to his Majeſty five rams and thirty-five ewes from her own flock, known by the name of Negretti, the reputation of which, for purity of blood and fineneſs of wool, is as high as any in Spain. For this preſent his Majeſty was pleaſed to ſend to the Marchioneſs in return eight fine Engliſh coach horſes.

Theſe ſheep, which were imported in the year 1792, have formed the baſis of a flock, now kept in the park of His Royal Highneſs the Duke of York at Oatlands, the breed of which has been preſerved with the utmoſt care and attention.

The wool of this flock, as well as that of the ſheep procured before from Spain, was acknowledged by the manufacturers who ſaw it, to be to all appearance of the very firſt quality; yet none of them choſe to offer a price for it at all equal to what they themſelves gave for good Spaniſh wool, leſt, as they ſaid, it ſhould not prove in manufacture ſo valuable as its appearance promiſed: it became neceſſary, therefore, that it ſhould be manufactured at the King's expenſe, in order that abſolute proof might be given of its actual fitneſs for the fabric of ſuperfine broad cloth; and this was done year after year in various manners, the cloth always proving excellent: yet the perſons to whom the wool was offered for ſale ſtill continued to undervalue it, being prepoſſeſſed with an opinion, that though it might not at firſt degenerate, it certainly ſooner or later would alter its quality much for the worſe.

In 1796 it was reſolved to ſell the wool at the price that ſhould be offered for it, in order that the manufacturers themſelves might make trial of its quality, although a price equal to its real value ſhould not be obtained: accordingly, the clip of that year was ſold for 2*s.* a pound, and the clip of the year 1797, for 2*s.* 2*d.*

The value of the wool being now in ſome degree known, the clip of 1798 was waſhed in the Spaniſh manner, and it ſold as follows:

The number of fleeces of ewes and wethers was 89;

Which produced in wool waſhed on the ſheep's backs	295 *lb.*
Loſs in ſcowering - - - - - -	92
Amount of ſcowered wool - - - - -	203

		lb.	*per lb.*	
Which produced,	Raffinos	167	at 5*s.*	47*l.* 8*s.*
	Finos	23	at 3*s.* 6*d.*	
	Terceros	13	at 2*s.* 6*d.*	

The clip of 1799 was managed in the ſame manner, and produced as follows:

The number of fleeces of ewes and wethers was 101;

Which produced in wool waſhed on the ſheep's backs	346 *lb.*
Loſs in ſcowering - - - - - -	92
Amount of ſcowered wool - - - -	254

		lb.	*per lb.*	
Which produced,	Raffinos	207	at 5*s.* 6*d.*	63*l.* 14*s.* 6*d.*
	Finos	28	at 3*s.* 6*d.*	
	Terceros	19	at 2*s.*	

The rams' wool of the two years ſorted, together produced as follows:

Quantity of wool waſhed on the ſheep's back	314 *lb.*
Loſs in ſcowering - - - - - -	99
Amount of ſcowered wool - - - -	215

		lb.	*per lb.*	
Which produced,	Raffinos	181	at 4*s.* 6*d.*	45*l.* 15*s.* 6*d.*
	Finos	22	at 3*s.* 6*d.*	
	Terceros	12	at 2*s.*	

It is neceſſary to account for theſe extraordinary prices by ſtating, that in the year 1799, when both ſales were effected, Spaniſh wool was dearer than it ever before was known to be; but it is alſo proper to add, that 5*s.* 6*d.* was then the price of the beſt Spaniſh piles; and that none were ſold higher, except, as it is ſaid, a very ſmall quantity for 5*s.* 9*d.*

The King has been pleaſed to give away to different perſons, who undertook to try experiments by croſſing other breeds of ſheep with the Spaniſh, more than one hundred rams and ſome ewes. In order, however, to make the benefit of this valuable improvement in the ſtaple commodity of Great Britain, acceſſible to all perſons who may chooſe to take the advantage of it, his Majeſty is this year pleaſed to permit ſome rams and ewes to be ſold, and alſo to command that reaſonable prices ſhall be put upon them, according to the comparative value of each individual; in obedience to which it has been ſuggeſted that five guineas may be conſidered as the medium price of a ram, and two guineas that of a ewe; a ſum which it is believed the purchaſer will in all caſes be able to receive back with large profit, by the improvement his flock will derive from the valuable addition it will obtain.

Though the mutton of the Spaniſh ſheep was always excellent, their carcaſes were extremely different in ſhape from that mould which the faſhion of the preſent day teaches us to prefer; great improvement has however been already made in this article, by a careful and attentive ſelection of ſuch rams and ewes as appeared moſt likely to produce a comely progeny; and no doubt can be entertained, that, in due time, with judicious management, carcaſes covered with ſuperfine Spaniſh wool may be brought into any ſhape, whatever it may be, to which the intereſt of the butcher, or the caprice of the breeder, may chuſe to affix a particular value.

Sir Joſeph Banks, who has the honour of being intruſted with the management of this buſineſs, will anſwer all letters on the ſubject of it, addreſſed to him in Soho-ſquare. The rams will be delivered at Windſor; the ewes at Weybridge, in Surrey, near Oatlands.

As thoſe who have the care of his Majeſty's Spaniſh flock may naturally be ſuppoſed partial to the project of introducing ſuperfine wool in theſe kingdoms, it has been thought proper to annex the following notice, in order to ſhow the opinion held of a ſimilar undertaking in a neighbouring country, where individuals, however they have miſtaken their political intereſt, are rather remarkable for purſuing and thoroughly weighing their own perſonal advantage in all their private undertakings, and for ſagacity in ſeizing all opportunities of improving, by public eſtabliſhments, the reſources of their nation.

FRENCH ADVERTISEMENT.

On the 24th of May laſt, an advertiſement appeared in the *Moniteur*, giving notice of a ſale of two hundred and twenty ewes and rams of the fineſt woolled Spaniſh breed, part of the flock kept on the national farm of Rambouillet; alſo two thouſand pounds of ſuperfine wool, being the preſent year's clip of this national flock; and one thouſand three hundred pounds of wool, the produce of the mixed breeds of ſheep kept at the Menagerie at Verſailles.

This advertiſement, which is official, is accompanied by a notice from Lucien Bonaparte, Miniſter of the Interior, as follows:

"The Spaniſh breed of ſheep that produce the fineſt wool, introduced into France thirty years ago, has not manifeſted the ſmalleſt ſymptom of degeneration: ſamples of the wool of this valuable flock, which was brought from Spain in the year 1786, are ſtill preſerved, and bear teſtimony that it has not in the leaſt declined from its original excellence, although the diſtrict where theſe ſheep have been kept is not of the beſt quality for ſheep-farming; the draughts from this flock, that have been annually ſold by auction, have always exceeded in value the expectation of the purchaſers in every country to which they have been carried, that is not too damp for ſheep.

"The weight of their fleeces is from ſix * to twelve pounds each, and thoſe of the rams are ſometimes heavier.

"Sheep of the ordinary coarſe-woolled breeds, when croſſed by a Spaniſh ram, produce fleeces double in weight, and far more valuable, than thoſe of their dams; and if this croſs is carefully continued, by ſupplying rams of the pure Spaniſh blood, the wool of the third or fourth generation is ſcarce diſtinguiſhable from the original Spaniſh wool.

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* This muſt mean fleeces unwaſhed, or in the yoke, as it is technically termed.

"Theſe mixed breeds are more eaſily maintained, and can be fattened at as ſmall an expenſe as the ordinary breeds of the country.

"No ſpeculation whatever offers advantages ſo certain, and ſo conſiderable, to thoſe who embark in it, as that of the improvement of wool, by the introduction of rams and ewes of the true Spaniſh race, among the flocks of France, whether the ſheep are purchaſed at Rambouillet, or elſewhere; in this buſineſs, however, it is of the greateſt importance to ſecure the Spaniſh breed unmixed, and the utmoſt precaution on that head ſhould be uſed, as the avarice of proprietors may tempt them to ſubſtitute the croſſed breeds inſtead of the pure one, to the great diſappointment of the purchaſer.

"The amelioration of wool at Rambouillet has made ſo great a progreſs, that in a circle from twenty-four to thirty-ſix miles in diameter, the manufacturers purchaſe thirty-five thouſand pounds of wool, improved by two, three, or four croſſes. Thoſe who wiſh to accelerate the amelioration of their flocks by introducing into them ewes of this improved ſort, may find abundance to be purchaſed in that neighbourhood at reaſonable rates."

VII. *On a periodical Variation of the Barometer, apparently due to the Influence of the Sun and Moon on the Atmoſphere. By* LUKE HOWARD, *Eſq. Read before the Aſkeſian Society, London* *.

THAT the moon and planets exerciſe a certain influence (excluſive of the more evident effects of the ſun) on the ſtate of the globe we inhabit, appears to have been the opinion of

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[*] This ſociety conſiſts of a ſelect number of gentlemen, aſſociated for their mutual improvement in the different branches of natural philoſophy. It was inſtituted in March 1796, and the regular meetings are held every other week during the winter.

The preſent eſſay is extracted, by permiſſion, from the collection of thoſe furniſhed at different times by the members; and from the ſame ſource we have been promiſed to be favoured with ſome other papers which may prove gratifying to our philoſophical readers.

mankind from the moſt remote antiquity. The true nature and extent of their influence ſeems, however, not to have been well underſtood until the epoch of the ſublime diſcoveries of Newton.

Whether the firſt profeſſors of aſtrology, a ſcience which we find to have been ſo antiently cultivated in the Eaſt, entertained more juſt ideas of the planetary influence than their ſucceſſors in later times, we may not now be able to determine. With reſpect to the pretenſions of the latter, it is, however, certain, that the maſs of abſurd notions, the product of credulity and imagination, under which is buried every thing true or valuable in their writings, has given occaſion to their falling into general contempt, more eſpecially in the preſent advanced ſtate of aſtronomy. The more myſterious and impoſing ſtudy of aſtrology has ſtill nevertheleſs its advocates; and the pretenders to it, availing themſelves of the meteorological facts recorded by their predeceſſors, and countenanced by ſomething not well underſtood in the experience of the multitude, continue to put forth their annual predictions of the weather; and, in this reſpect, in ſpite of ſufficiently frequent proofs of this want of *ſpecial* foreſight, they ſucceed often enough to keep up a ſort of credit with their wondering readers. In the mean time, is it any credit to thoſe who have long had in their hands correct regiſters of the weather, and other means of forming an accurate judgment of ſuch matters, that they either ſuffer theſe deceptions to paſs unnoticed, or content themſelves with ſubſtituting ridicule for inquiry?

Obſerving that the ſubject of lunar influence on the atmoſphere promiſes at length to obtain due conſideration (by ſome paſſages in foreign journals, as the Theory given by Lamarck in the *Journal de Phyſique, Floréal, An.* 8, *&c.**), I am inclined to bring forward ſome obſervations which firſt occurred in keeping the regiſter of the barometer for 1798 annexed, and which have been amply confirmed in extending the inquiry to other regiſters.

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* In particular a paper by Toaldo, in the *Journal des Sciences utiles*, tranſlated in the Philoſophical Magazine, Vol. III. p. 121, with which the reader is requeſted to compare theſe obſervations (which were written previous to the author's knowledge of Toaldo's Theory), as they mutually ſupport each other.

The dotted line, which, in the barometric chart* (Pl. IX.) repreſents the courſe of the barometer, was traced by daily obſervations (with an excellent barometer made by Haas) on a ſet of blank charts, each of which ſerves for a month; being ruled horizontally with red lines, dividing the ſpace into inches and tenths, and ſerving for a ſcale, and perpendicularly with others, two-tenths diſtant, one of which is allotted to each day †.

My regiſter was kept at Plaiſtow in Eſſex, about five miles eaſt of London, except three ſpaces diſtinguiſhed by full dots at each extremity, which were noted in London at an elevation not much more than twenty feet greater, and for moſt part of which I am indebted to the obſervations of a friend. As the barometer rarely changes its *direction* during the night, it is evident that, by an enlarged ſcale and frequent obſervations, a very accurate account of its variations might be kept in this way; and none can give a more pleaſing ſynoptical idea of theſe, with the correſponding changes of weather, for the paſt month or year.

As the moon's phaſes had been inſerted in the explanatory plate of Dr. Buxton, by a character affixed to the day, I continued this practice, and after ſome time began to ſuſpect a coincidence between theſe and the courſe of the barometer, which at length became very evident, and gave occaſion to further inquiry. This coincidence conſiſts in the depreſſion of the barometrical line on the approach of the new and full moon, and its elevation on that of the quarters. In above thirty out of the fifty lunar weeks in this year, the barometer will be found to have changed its *general direction* once in each week, in ſuch manner as to be either riſing or at its *maximum* for the week preceding and following, about the time of either quarter; and to be either falling or at its *minimum* for the two weeks, about the new and full. It is remarkable, that the point of greateſt depreſſion during the year, *viz.* to 28,67, is found about twelve hours after the new moon on the 8th of the eleventh month; and that of its greateſt and extraordinary elevation to 30,89 on the 7th of the ſecond month at the time of the laſt quarter. Moreover, this coincidence appeared to obtain the moſt regularly in fair and moderate weather; and, in general, when the barometer fell during the interval between the new or full moon and the quarters, an evident perturbation in the atmoſphere accompanied; of which may be inſtanced II. 15 to 23, when the barometer, after an uncommon riſe, continued to fall rapidly after the new moon, with ſevere cold, which ended ſuddenly in ſtormy and wet weather; again, VI. 14 to 20, when two weeks of fair weather ended in a thunderſtorm. In the greater part of XII. the uſual coincidence diſappears, and the *converſe* takes place, the barometer being low at the quarter and high at the full, amidſt continued alternations of rain, froſt, and ſnow; and, for part of the time, high winds. The remarkable depreſſion between the 24th and 28th was attended with intenſe cold, the thermometer being noted on four ſucceſſive days at 15°. 14°. 9°. 13°. reſpectively. On the two days preceding the laſt quarter, the barometer roſe rapidly, and rain followed!

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* The accompanying plate is engraved on a ſcale of half an inch to an inch.

† The blank charts are ſold by Edward Nairne, Cornhill, and were publiſhed by Dr. Buxton in 1794, who has obliged me with the inſpection of his regiſter from that time to the preſent. There are columns added for wind, rain, &c. which it was not neceſſary to introduce on this occaſion.

In the ſeventh month, which was wet and windy throughout nearly, there is the leaſt of this coincidence to be ſeen. Inſtances may be obſerved in which the tendency to riſe or fall at the times pointed out, after being interrupted by an occaſional cauſe, is reſumed. On the whole I thought there appeared ſufficient ground, on the evidence of the year 1798, to ſuppoſe that the gravity of our atmoſphere, as indicated by the barometer, may be ſubject to certain periodical changes, effected by a cauſe more ſteady and regular than either change of temperature, currents, or ſolution and precipitation of water, to which I believe the whole variation has been heretofore attributed. To diſcover theſe periodical changes, if poſſible, and to aſcertain their amount, independently of the operation of the laſt-mentioned cauſes, I took the following method: Paſſing regularly through the regiſter of the barometer which I had choſen for examination, I extracted one obſervation for each lunar week, as near as poſſible to the time of the change, which I ranged under its proper title, either new moon or full, &c. Having gone through the year, I took the mean of the whole of theſe notations, and afterwards the mean of thoſe under the title of new moon, full moon, firſt and laſt quarter, reſpectively; by compariſon of which with the general mean I attained my object.

The annexed regiſter being examined thus, by taking the numbers marked on the plate under each change, the reſults were as follows:

Mean of the Whole.	Full Moon.	Laſt Quarter.	New Moon.	Firſt Quarter.
29,9638	29,906	30,153	29,719	29,980
Difference - -	—,0578	+,1892	—,2448	+,0262

It will appear, by inſpecting the plate, that if the numbers had been taken with a latitude of only 36 hours on either ſide the moon's changes, the reſults would have been ſtill more favourable to the ſuppoſed coincidence.

The regiſter of the Royal Society, given in the Philoſophical Tranſactions, affording an opportunity of trying it on a much larger ſcale, I extracted weekly obſervations for ten years, *viz.* from 1787 to 96, adhering ſtrictly to the rule I had laid down, and compared the mean of each claſs with the mean of the whole for ten years, as alſo with the mean of the regiſter at large for that time, which gave the following reſults.

Mean of the Whole.	New Moon.	Firſt Quarter.	Full Moon.	Laſt Quarter.
29,818	29,7946	29,8910	29,7812	29,8823
Difference from the Total Mean.	—,0234	+,0730	—,0368	+,0643

The mean of the regiſter at large appeared (on computation from the mean of each year as given in the regiſter) to be 29,89, whence it appears that the depreſſions at the new and full moon either amounted to more, on the whole, than the elevations at the quarters, or that they fell out nearer to the time. I was quite ſatisfied, in paſſing through this regiſter, that if I had allowed myſelf to chooſe the higher notations about the quarters, and the lower about the new and full, with a latitude of 24 or 36 hours, it would have made the reſults as much more favourable to my concluſions as in her former caſe.

Now, to omit the conſideration of other proofs for the preſent, it appears to me evident, that the atmoſphere is ſubject to a periodical change of gravity, whereby the barometer, on a mean of ten years, is depreſſed at leaſt one-tenth of an inch while the moon is paſſing from the quarters to the full and new; and elevated, in the ſame proportion, during the return to the quarter. To what cauſes ſhall we attribute this periodical change, other than the attraction of the ſun and moon for the matter compoſing the atmoſphere?

The atmoſphere is a gravitating fluid, differing, in a phyſical ſenſe, from the water, chiefly in poſſeſſing leſs gravity; and it is demonſtrable *à priori* on the principles of the Newtonian philoſophy, that it ought to have its tides as well the ocean, although in a degree as much leſs perceptible as is its gravity.

I ſuppoſe, therefore, that the joint attractions of the ſun and moon at the new moon, and the attraction of the moon predominating over the ſun's weaker attraction at the full, tend to depreſs the barometer, by taking off from the gravity of the atmoſphere, as they produce a high tide in the waters, by taking off from their gravity: and again, that the attraction of the moon being diminiſhed by that of the ſun at her quarters, this diminution tends to make a high barometer, together with a low tide, by permitting each fluid to preſs with additional gravity upon the earth.

I am aware that ſeveral circumſtances attending the facts I have brought forward prevent them from making a caſe fairly parallel with the tides; on which I ſhall make ſuch remarks as occur in this early ſtage of the inquiry.

In the firſt place, the waters have daily tides correſponding with the motions of the earth and moon; and there is additional elevation and depreſſion about the phaſes of the moon, amounting, ſuppoſe, to a fourth part of the whole.

In the atmoſphere we have at preſent no proof of diurnal tides, which ought to be the moſt apparent *.

The elevations and depreſſions of the barometer, *which appear to be periodical*, are ſometimes more, ſometimes leſs, conſiderable; but in moſt caſes, when they are regular, greatly exceed the proportion which they ought to bear to the extremes of the ſcale, on the ſuppoſition of their being due to the relative poſitions of the ſun and moon only. They arrive at their extent ſometimes before, ſometimes after, the time of the moon's phaſes, and their direction is even ſometimes contrary to the theory propoſed, for many days together; in all which particulars they diſagree with the tides of the ocean.

Now in order to a proper conſideration of theſe differences, it is neceſſary to keep in view the different conſtitution of the two fluids, which are the ſubjects of the compariſon.

The ocean is a denſe fluid, incumbent on the ſolid earth, pretty much alike in temperature and compoſition throughout, ſubject indeed to certain currents which are found to be pretty conſtant and appreciable. It does not appear to vary in quantity. It has a well-defined ſurface, by which we can meaſure the alterations of level it is ſubject to:

The atmoſphere is a much rarer and an elaſtic fluid, incumbent partly on tracts of land, of various qualities, partly on the moveable ſurface of ſeas, partly on ſnow and ice. It differs, at different times, in quantity, and varies much in denſity, temperature, and compoſition; is moveable through its whole extent by different and oppoſite currents, for the moſt part uncertain and changeable; and we know nothing about its ſurface. Theſe circumſtances being conſidered, we ſhall not expect, even in theory, the ſame regularity in the tides of the atmoſphere as in thoſe of the ocean.

The want of facts to prove the exiſtence of diurnal tides appears indeed at firſt view an inſuperable difficulty; ſince, if theſe did take place, the barometer ought to indicate them,

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* The means of aſcertaining theſe were not ſo obvious in the time of Newton; and it is accordingly ſaid, in B. II. chap. 6, of Pemberton's Newton, that "the gravitation of theſe fluids (water and air) in the earth towards the moon produces no ſenſible effect, except in the ſea, where it cauſes the tides."

by riſing and falling twice in each day, in a degree proportioned to the ſuppoſed weekly tide. But perhaps even this difference may be found to reſult from the different conſtitution of the two fluids. The water, being of more uniform denſity and poſſeſſing ſo much greater gravity, is elevated and ſubſides with proportionate ſteadineſs. The air, falling greatly ſhort of the water in gravity, and its denſity continually decreaſing upwards, muſt needs be very differently affected. If we take notice alſo of the greater eaſe with which the latter is thrown into currents, and laſtly of its remarkable property of diſſolving mere water as it increaſes in denſity and temperature, and letting it fall as it decreaſes in either, it will not ſeem impoſſible, that the daily flux and reflux, which is ſo manifeſt in the waters, ſhould be loſt and counteracted in the atmoſphere; while, on the other hand, the progreſſive increaſe of either, through the lunar week, ſhould, by the very ſame cauſes, be ſometimes promoted and rendered more conſpicuous.

It will, however, be ſoon enough to enter upon the theory of the atmoſpherical tides, when the preceding facts ſhall have been examined, and the influence of the ſun and moon on the gravity of the atmoſphere eſtabliſhed, by more extenſive obſervation. It is for this purpoſe that the ſubject is now brought forward, and the cooperation of meteorological obſervers, in this or other countries, into whoſe hands this may come, is requeſted. The coincidence, ſo far as hitherto obſerved, is an important fact, and ſhould it be found to obtain generally, it will neceſſarily lead to ſeveral important conſequences; and, in the firſt place, to a new and more ſatisfactory theory of the barometer; in conſequence of which that inſtrument may be more ſucceſsfully applied to foretell the changes of the weather. The true reaſon likewiſe of the weather ſo frequently coinciding, in the time of its various alterations, with the changes of the moon (a coincidence which has long ſerved to direct the predictions of the almanac-makers), will be apparent, and the diligent meteorologiſt will avail himſelf of it in forming *probable conjectures* on the changes likely to enſue for a certain time, not exceeding that which limits the operation of the known cauſe or cauſes.

P. S. That the exiſtence of a daily flux and reflux in the atmoſphere has not been hitherto generally admitted, may be in part owing to the want of frequent obſervations upon inſtruments with a ſufficient range of ſcale. It is, however, more probable, that the lunar weekly flux or reflux, as either prevails, may have the greateſt ſhare in preventing the detection of the contrary ſemi-diurnal movement; for, at Calcutta, where the range of the barometer is at all times inconſiderable (if we may judge by one year's obſervations), and where the weekly lunar influence can ſcarcely be traced, a manifeſt regular daily tide has been detected by the diligent obſervations of Francis Balfour, Eſq. in 1794, which took place, as follows, during the month called April:—Beginning from ſix in the morning, the barometer roſe for four hours, then fell during eight hours; then roſe four hours, and fell eight again; which took place daily, and, with very little exception, uninterruptedly.—(See the Regiſter in the Aſiatic Reſearches.)

VIII. *Letter from* C. H. TATHAM, *Eſq. Architect, containing a brief Account of the grand antique Bacchanalian Vaſe, late in the Poſſeſſion of the Right Hon. Lord Cawdor, now at Woburn Abbey.*

SIR, September 15, 1800.

THE attention of the amateurs of the fine arts having been lately excited by the ſale of Lord Cawdor's noble collection of antiques, and particularly of the celebrated Bacchanalian vaſe, formerly an object univerſally admired in the Lanti Palace at Rome, the following brief account of it may not be unacceptable to your readers.

The forms of all antique vaſes, whether bell-ſhaped or the tazza, are ſuppoſed to have been firſt taken from the calyx of the lotus; the latter repreſenting the plant in a flatter form, as it appears when fully blown, and the former in the more early ſtage of inflореſcence.

The lotus is a celebrated water-plant, well known in Upper Aſia, which, from its ſtructure, and its reputed quality of being generated by heat and moiſture, was ſelected by the antients as the ſymbol of the generation and procreation of the human ſpecies; it was therefore deified and held ſacred, and became the model from which, as it is thought, they copied the form of their principal vaſes.

The grand Bacchanalian vaſe in queſtion (ſee a geometrical elevation of it, Plate X.) is of the lotus form, bell-ſhaped, and was moſt probably conſecrated to the god Bacchus, as may be concluded from the finely-ſculptured Bacchanalian maſks, and other features that accompany it: it muſt therefore have been uſed either as a laver or ſymbol only of this part of the heathen mythology, and for no other uſe; for it is certain that no wine was ever poured into it.

This ſuperb monument of antique decoration was dug up, ſome centuries ago, among the ruins of Adrian's villa, together with the fragments of three other vaſes of nearly ſimilar dimenſions; all of which appeared, by the ſituation in which they were found, to have occupied the ſame particular ſpot of that once extenſive and magnificent emporium of art. It was then removed to the villa Lanti, near Rome, where for many years it attracted the notice and excited the admiration of both the traveller and the artiſt. This, and one at Warwick caſtle, which is ſomewhat more decorated, are the only complete vaſes, of the ſame dimenſions, extant; and are unqueſtionably the moſt magnificent and nobly-ſculptured ſpecimens of antique decoration of this kind ever diſcovered.

The Lanti vaſe was brought from Rome about twelve years ago, at a conſiderable riſk and expenſe, by the Right Hon. Lord Cawdor, on whoſe claſſical taſte and judgment it muſt ever confer the higheſt credit. The removal of this grand work of art from that city, cauſed great jealouſy among the ſuperintendants of the Vatican Muſæum, then forming under the auſpices of the reigning pontiff, the late Pius the Sixth; who, it is well known, in his reſentment on this occaſion, threatened ſeveral perſons concerned in the removal of the vaſe, with the gallies.

The dimenſions of the vaſe are: diameter of the mole, ſix feet three inches; height, with its preſent plinth, ſix feet nine inches. It was originally in one piece of Parian marble.

It was purchaſed at the public ſale of Lord Cawdor's muſæum in Oxford-ſtreet, the 6th of June 1800, for ſeven hundred guineas, and is now in the poſſeſſion of the Duke of Bedford, who has cauſed it to be removed to Woburn Abbey, where it is to be placed in an appropriate ſituation, in the centre of an extenſive and beautiful greenhouſe, deſigned and executed for his Grace by Mr. Holland.

I am, Sir,
Yours, &c.
CHARLES HEATHCOTE TATHAM.

To the Editor of Philoſophical Magazine.

NEW PUBLICATIONS.

New Obſervations concerning the Inflexions of Light, accompanying thoſe of Newton, but differing from his, and appearing to lead to a change of his Theory of Light and Colours. Cadell and Davies; 1799. 134 Pages, with Eight Copperplates.

THE author of this eſſay has carefully repeated thoſe experiments by which Sir Iſaac Newton effected his analyſis of light. The experiments have produced to his obſervation, phenomena materially different from thoſe which appeared to Newton. He ſeems to have obſerved the phenomena with very accurate attention. He concludes from the whole, that Newton has erred in believing every ray of common light to be compoſed of ſeven differently coloured, primigenial, elementary rays. On the contrary, this author infers from his obſervations, that *all light is originally of one uniform white colour*; that *its diverſity of colours, in inflexion, is occaſioned by the bendings, ſeparations, and other changes of its parts in paſſing through a tranſparent medium, or under attraction by the edge, angle, or ſide of an approaching body*; and that, by conſequence, the Newtonian Theory of light and colours is not fundamentally true.

The apparent accuracy of theſe obſervations; the logical fairneſs fitneſs of the induction; the literary compoſition of the eſſay, deſerve every praiſe. Without having ourſelves repeated the experiments, and without knowing them to have been repeated, with ſimilar reſults, by others, we would not preſume to decide concerning the truth of the doctrine.

The ſame doctrine has been more haſtily and obſcurely ſuggeſted by Mr. Heron, in his Elements of Chemiſtry.

The Philoſophical Tranſactions of the Royal Society of London for 1800. *Parts I. and II.* Elmſly, London.

THE Firſt Part contains: 1. The Croonian Lecture. On the Structure and Uſes of the Membrana Tympani of the Ear. By Everard Home, Eſq. F.R.S.—2. On the Methods of determining, from the real Probabilities of Life, the Nature of contingent Reverſions, in which three Lines are involved in the Survivorſhip. By William Morgan, Eſq. F. R. S.—3. Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Eſq.—4. On the Power of penetrating into Space by Teleſcopes, with a comparative Determination of the Extent of that Power in natural Viſion and in Teleſcopes of various Sizes and Conſtructions: illuſtrated by ſelect Obſervations. By William Herſchel, LL.D. F.R.S.—5. A ſecond Appendix to the improved Solution of a Problem in Phyſical Aſtronomy, inſerted in the Philoſophical Tranſactions for the Year 1798, containing ſome further Remarks, and improved Formulæ, for computing the Co-efficients A and B, by which the Arithmetical Work is conſiderably ſhortened and facilitated. By the Rev. John Hellins, B.D. F.R.S. and Vicar of Potter's Pury, in Northamptonſhire.—6. Account of a Peculiarity in the Diſtribution of the Arteries ſent to the Limbs of ſlowly-moving Animals; together with ſome other ſimilar Facts By Anthony Carliſle, Eſq.—7. Outlines of Experiments and Inquiries reſpecting Sound. By Thomas Young, M.D. F.R.S.—8. Obſervations on the Effects which take place from the Deſtruction of the Membrana Tympani of the Ear. By Mr. Aſtley Cooper.—9. Experiments and Obſervations on the Light which is ſpontaneouſly emitted, with ſome degree of permanency, from various Bodies. By Nathaniel Hume, M.D. F.R.S. and A.S.—10. Account of a Series of Experiments undertaken with a View of decompoſing the Muriatic Acid. By Mr. William Henry.—11. On a new Fulminating Mercury. By Edward Howard, Eſq. F.R.S.—Appendix. Meteorological Journal kept at the apartments of the Royal Society, by order of the Preſident and Council.

The Second Part contains: 12. On double Images cauſed by atmoſpherical Refraction. By William Hyde Wollaſton, M.D. F.R.S.—13. Inveſtigation of the Powers of the priſmatic Colours to Heat and Illuminate Objects; with Remarks that prove the different Refrangibility of radiant Heat. To which is added, an Inquiry into the Method of viewing the Sun advantageouſly with Teleſcopes of large Apertures and high magnifying Powers. By William Herſchel, LL.D. F.R.S.—14. Experiments on the Refrangibility of the inviſible Rays of the Sun. By William Herſchel, LL.D. F.R.S.—15. Experiments on the ſolar and on the terreſtrial Rays that occaſion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occaſion them, are ſubject, in order to determine whether they are the ſame, or different. By William Herſchel, LL.D. F.R.S.—Chemical Experiments on Zoophytes; with ſome Obſervations on the component Parts of Membrane. By Charles Hatchett, Eſq. F.R.S.—17 On the Electricity excited by the mere contact of conducting Subſtances of different Kinds. In a Letter from Mr. Alexander Volta, F.R.S. Profeſſor of Natural Philoſophy in the Univerſity of Pavia, to the Right Hon. Sir Joſeph Banks, Bart. K.B. P.R.S.—18. Some Obſervations on the Head of the Ornithorhynchus paradoxus. By Everard Home, Eſq. F.R.S.

INTELLIGENCE,

AND

MISCELLANEOUS ARTICLES.

LEARNED SOCIETIES.

FRENCH NATIONAL INSTITUTE.

ON the 4th of July laſt, the following account of the labours of the Mathematical and Phyſical Claſs during the preceding three months, was read by C. Cuvier, ſecretary:

The antients admitted only four elements, by the combination of which they ſuppoſed all other terreſtrial ſubſtances to be formed. Modern chemiſtry has rejected this vulgar opinion, and has given the name of *elementary* to all ſubſtances which it is not able to decompoſe, but it at the ſame time endeavours to diminiſh the number of them; and whenever it is able to prove that a ſubſtance, which appears to be ſimple, reſults from the combination of two others, it obtains the double advantage of ſimplifying the theory of the ſcience, and of being able to reproduce, at pleaſure, that ſubſtance, with the analyſis of which it is acquainted.

It appears at preſent that it is on the eve of a diſcovery of this kind in regard to two ſubſtances, very intereſting on account of the part which they act throughout nature, and of the uſe to which they are applied in the arts. I here allude to the acid of ſea-ſalt, called by the modern chemiſts the *muriatic acid*; and the two fixed alkalies, *ſoda* and *potaſh*.

It has lately been diſcovered, that almoſt all the *acids* are obtained from the combuſtion of certain ſubſtances; the ſubſtance which muſt be burned to obtain an acid is called the *radical* of that acid, that is to ſay, according to the pneumatic theory, which muſt be combined with oxygen to produce that acid. The *marine acid* was one of thoſe the radical of which was not known; and it is this radical which Berthollet has been endeavouring to diſcover.

This chemiſt thinks himſelf authoriſed to believe that the muriatic acid is a triple compound of azot and a ſmall quantity of hydrogen and oxygen. It would be tedious to give an account of the various and accurate experiments which conducted him to this reſult. It will be ſufficient to ſay in general, that he always ſaw muriatic acid formed, when to the preſence of the nitric acid there was joined any circumſtance which forced the water to be decompoſed. But the radical of the nitric acid is known to be azot, and water can add only hydrogen to the principles of that acid. It was to an induction nearly of the ſame kind, that Berthollet was indebted ſome years ago, for his diſcovery of the compoſition of *volatile alkali* or *ammonia*, which he found to be formed of *hydrogen* and *azot*.

Chemiſts then employed themſelves in endeavouring to diſcover the compoſition of the two fixed alkalies. It was natural to think that one at leaſt of the two principles of the volatile alkali would be common to them and deſerve the name of *alcaligen*, as *dephlogiſticated* air has been called *oxygen* becauſe it is the common principle of all the acids. But on one hand they were ignorant whether the azot or the hydrogen was the common principle of the alkalies, and on the other, they did not know with what ſubſtances it ought to be combined to produce them.

Guyton has preſented ſome experiments made by Deſormes, which he has in part repeated, and which tend to prove that *potaſh*, or fixed *vegetable alkali*, is a compound of *hydrogen* and *lime*. Theſe chemiſts have ſeen lime produced under circumſtances when, in every inſtance of all the bodies brought into contact, none but the *potaſh* could contain it. They have ſeen alſo that this production of lime was preceded by the combuſtion and diſappearance of the hydrogen.

Soda or fixed mineral alkali having given magneſia under ſimilar circumſtances, they think that this alkali is a combination of *hydrogen* with that earth. Deſormes goes much further, for he is of opinion that alumine, magneſia, and lime, are only the ſame earth combined with more or leſs azot,

azot. Were his ideas confirmed, we ſhould not only approach near to the doctrine of the antients reſpecting the elements, but have an eaſy explanation of a multitude of phenomena difficult to be accounted for in the hiſtory of nature; as, the ſaltneſs of the water of the ſea; the formation of nitre and marine ſalt in inhabited places; that of potaſh in vegetables, of ſoda in animals; the converſion of ſome of the earths into each other, &c. All theſe facts, ſo myſterious in appearance, and which have exerciſed the ingenuity of the philoſophers for ſo many ages, would be conſequences eaſy to be deduced from theſe principles. Unfortunately, however, theſe chemiſts have not confirmed their analyſes by ſyntheſis; that is to ſay, they have not yet remade potaſh by combining directly lime and hydrogen.

The labour of Berthollet on the muriatic acid had been preceded and occaſioned by another on an object no leſs important—*eudiometry*. This is the name given to the art of analyſing atmoſpheric air, and in particular of aſcertaining how many parts of pure vital air or oxygen the atmoſphere of any place contains. The health of mankind as well as philoſophy is intereſted in the perfection of this art. The combuſtion of phoſphorus has been long employed for this analyſis, becauſe this combuſtion abſorbs the oxygen in a concrete acid, and it is then eaſy to meaſure what remains under a gaſeous form, which is azot.

Mr. Humboldt, a learned German philoſopher, thought he found this method defective, becauſe, according to his account, there always remained oxygen not fixed, and becauſe a portion of azot mixed itſelf with the acid. He entertained the like opinion of liver of ſulphur, or alkaline hydrogenated ſulphuret, which ſome employ alſo for the ſame uſe, and prefers the means pointed out by Fontana, or nitrous gas, which is an aëriform combination of azot, with leſs oxygen than is neceſſary to form nitric acid. When this gas is mixed with new oxygen, it abſorbs it more exactly, according to Mr. Humbolt, than the above ſubſtances; but as it may contain a variable quantity of azot, this mean is not unattended with inconveniences. Mr. Humboldt thinks that this might be remedied by examining each time the gas to be uſed, by waſhing it with a ſolution of copperas, or ſulphat of iron; which, according to his opinion, will abſorb all the nitrous gas, but leave the azot, which is mixed with it.

It is this action of the ſulphat of iron, or nitrous gas, with which Berthollet has been occupied; and it was on it indeed that the ſolution of the problem depended. He has found that under this circumſtance, as well as under a multitude of others, the nitrous gas is not only abſorbed, but decompoſed, and that it abandons a part of its azot to convert itſelf into nitrous acid. He is of opinion that the nitrous gas may differ in the proportions of its two principles, oxygen and azot: but that it does not contain azot in ſimple mixture.

Theſe intereſting reſults are the conſequences of the author's ideas on chemical affinities, communicated to the public above ſix months ago. He has made a new application of them by comparing the action of the metallic oxyds on ſubſtances, ſuch as acids, the compoſition of which is invariable; and he has proved that the action of theſe oxyds is modified by their ſtate of oxydation; that the more they are oxydated, for example, the leſs affinity they have for the nitric and ſulphuric acids, while a contrary effect takes place with regard to the muriatic acid. Beſides, the combinations of theſe oxyds with other ſubſtances depend alſo, whatever may be the ſtate of their oxydation, on the proportion in which they enter into theſe combinations. All the phenomena therefore depend on the proportions, and not on the conſtant affinity peculiar to each metal.

Three memoirs have been preſented on natural hiſtory; one of theſe, by Lacepede, contains ſome obſervations on the ant-eater, an American animal, which lives only on ants. The author has rectified the deſcription given by Buffon of the ſecond ſpecies of this animal, called *tamandua*, and deſcribed a blackiſh variety hitherto undeſcribed.

The ſecond memoir is on the ibis of the antients, by Cuvier. The author has diſcovered from mummies, monuments, and the deſcriptions of Homer and Plutarch, that this bird was not the ſame as that which modern naturaliſts

have

have conſidered as the ibis, but another ſpecies which he deſcribes.

The third memoir is by Haüy on a variety of martial pyrites, or ſulphat of iron, to which the author gives the name of *triacontaedra*, becauſe its cryſtals have thirty faces, ſix of which are rhombs and twenty-four trapezoids. Haüy has explained the laws of drecrement from which theſe cryſtals reſult, and demonſtrate ſome curious properties of this ſolid, to which geometricians hitherto have paid little attention.

The ſtructure of the earth, and the reſpective poſition of the materials of which it is compoſed, are among thoſe curious points of natural hiſtory ſtill involved in conſiderable obſcurity. We can ſtudy only the outer cruſt of the globe, and even this cruſt is attended with many difficulties. Happily for our curioſity, the nucleus in the high chains of mountains ſeems, by ſwelling up, to have burſt the matter by which it is enveloped, and to have ſhown itſelf uncovered. The labours of a Sauſſure, a Deluc, a Pallas, and a Dolomieu, have confirmed that the greater part of the large chains are compoſed of ſmall parallel chains, of which that in the middle, commonly the higheſt, conſiſts of granite, the two collateral ones conſiſt of ſchiſt and ſtones of an analogous kind, and the outer ones of calcareous ſubſtances. This general rule, which throws the greateſt light on the theory of the earth, did not ſeem to be applicable to the Pyrennees; there every thing ſeemed to be without order. The higheſt ſummits of Mount Perdu and le Marboré are certainly calcareous, and, as ſome aſſert, contain petrified ſhells. C. Ramond, by laborious journeys and ingenious deductions, has diſcovered the ſource of this embarraſſment. It ariſes from the ſmall chains being in an oblique direction, and from the ſchiſtous and calcareous bands ſituated towards Spain, being not only higher than thoſe of the ſame nature towards France, but ſurpaſſing even the granitic band, which forms the axis of the chain; ſo that the ridge which determines the fall of the ſtreams is different from the geological ridge.

The claſs in its laſt ſittings has been occupied with two objects of great importance for the public proſperity: the introduction into France of a new kind of domeſtic animal; and the means of making ſugar from indigenous plants.

The buffalo, originally a native of the warm and marſhy countries of Aſia and Africa, was introduced into Italy about the fourth century. Though ſmaller than the ox, it is more vigorous; the milk of the female is more abundant than that of the cow, and yields more butter and cheeſe. Every part of its body may be employed in the arts or for the purpoſes of life; and it poſſeſſes this advantage, that it can live in bogs, and feed on aquatic plants, which oxen and horſes refuſe. Its introduction, therefore, into France, will enable the farmers to turn to advantage a great number of marſhes, which at preſent are entirely uſeleſs for want of animals capable of living in them. The poſſeſſion of this animal is one of the benefits for which France is indebted to its firſt conqueſt of Italy. A part of the buffaloes brought to France has been killed by the peaſants, and another was neglected by thoſe to whom they were intruſted; but the remainder are in good condition, and in full increaſe, at the rural eſtabliſhment of Rambouillet, where they have been examined by Teſſier, Huzzard, and Buniva.

The art of extracting ſugar from European plants is a foreign diſcovery. M. Achard, of the Academy of Berlin, ſeems to have carried it very near to perfection. The National Inſtitute is anxious to aſcertain the reality of it, and Deyeux is about to communicate the reſults which have been obtained by the French chemiſts.

FULMINATING SILVER.

Mr. Howard has juſt diſcovered a new fulminating ſilver. We underſtand the proceſs for preparing it is very ſimilar to that followed in preparing his fulminating mercury; but the ſilver, it is thought, detonates with greater force.

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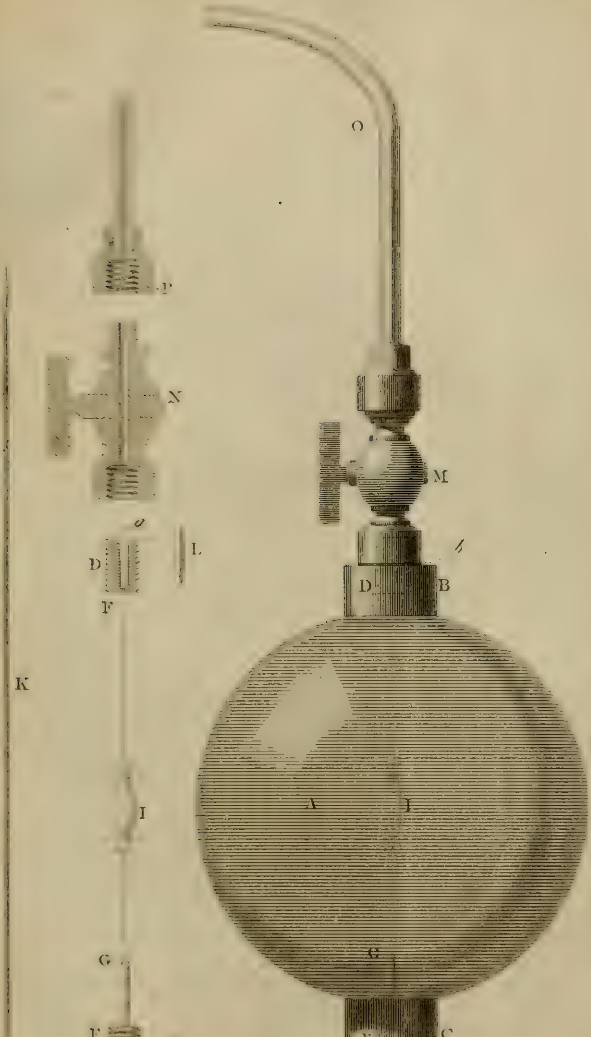

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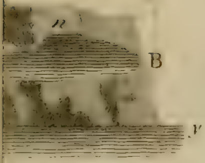

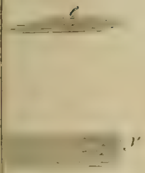

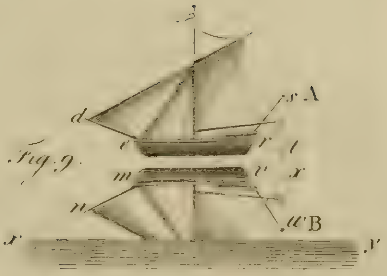
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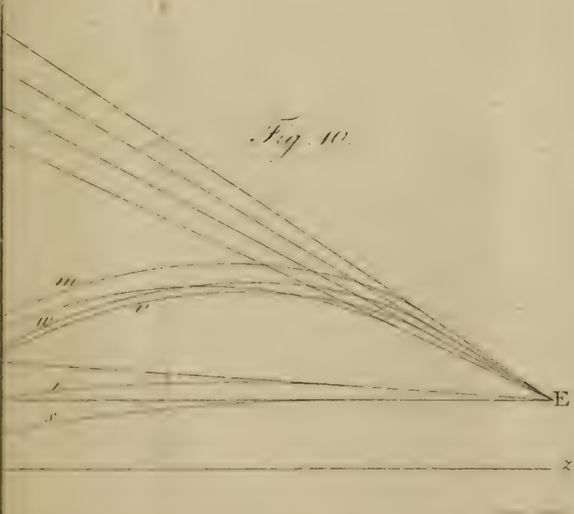
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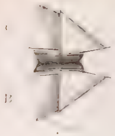

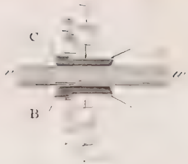

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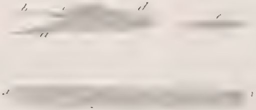

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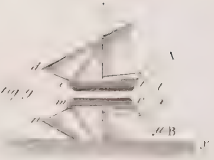

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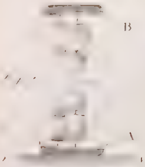

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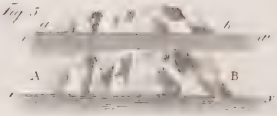

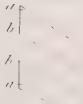

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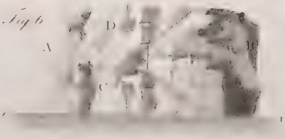

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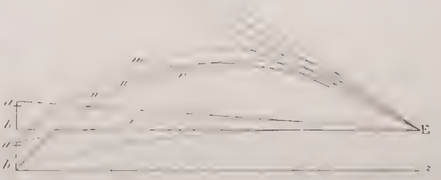

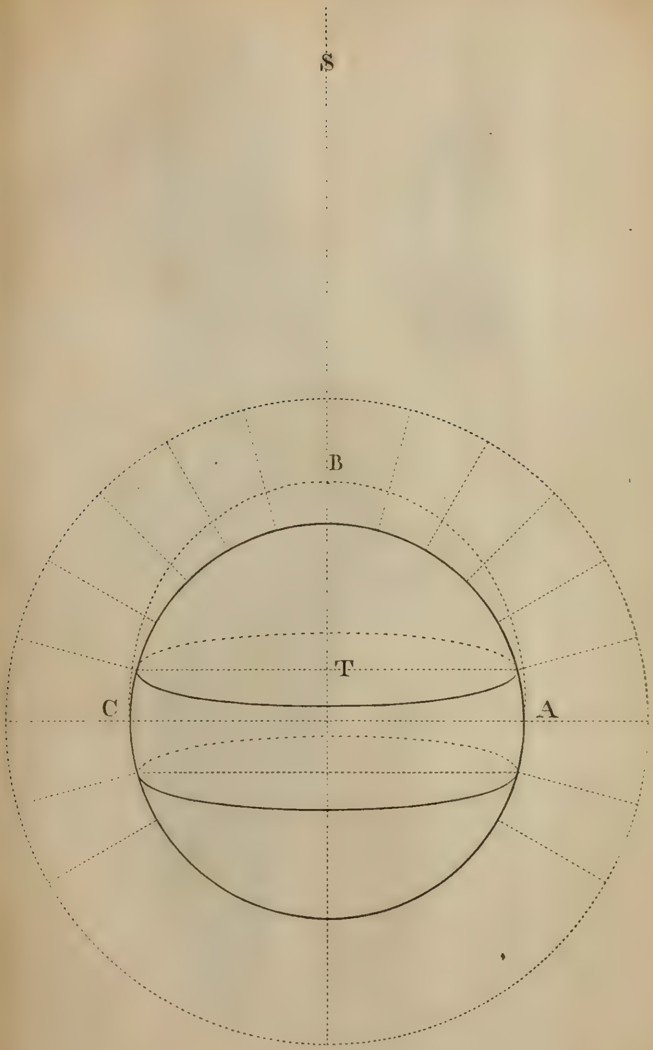
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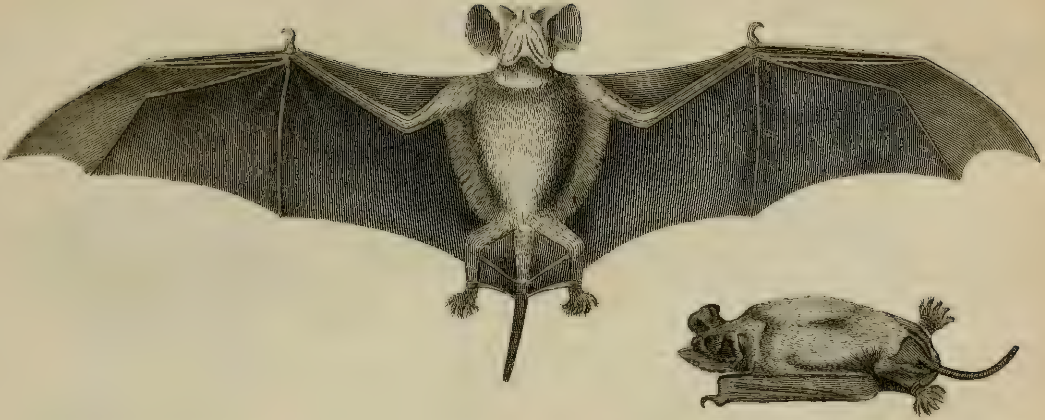

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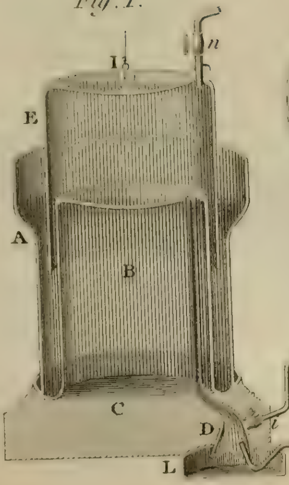

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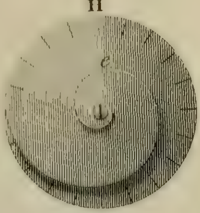

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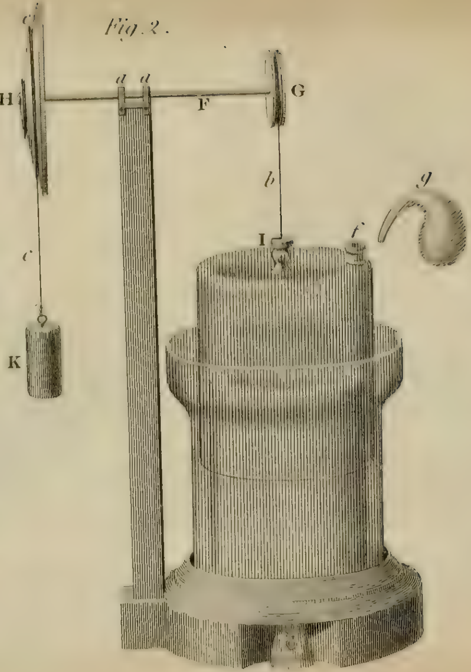

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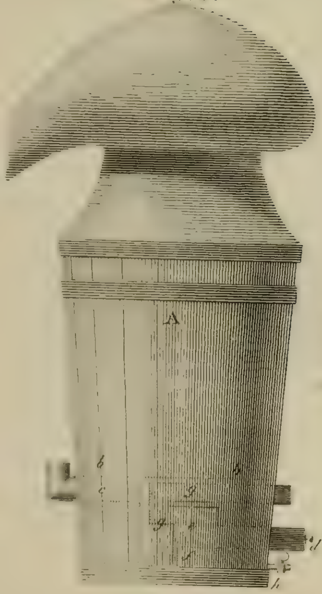

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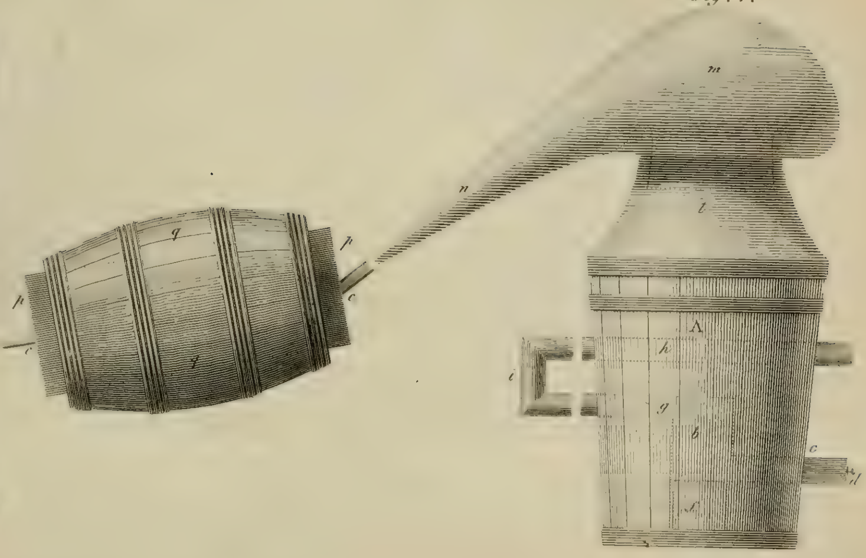

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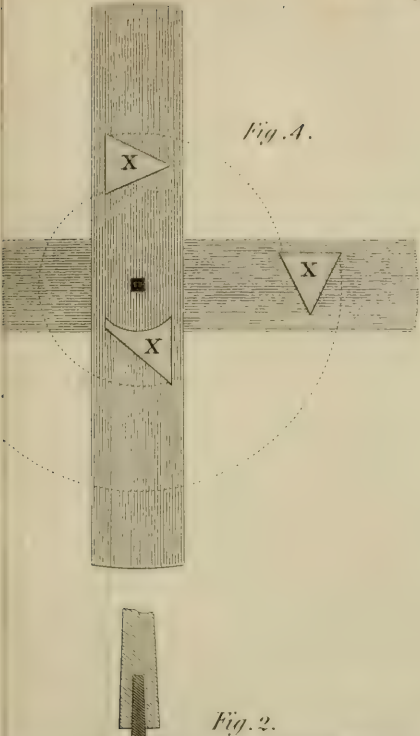

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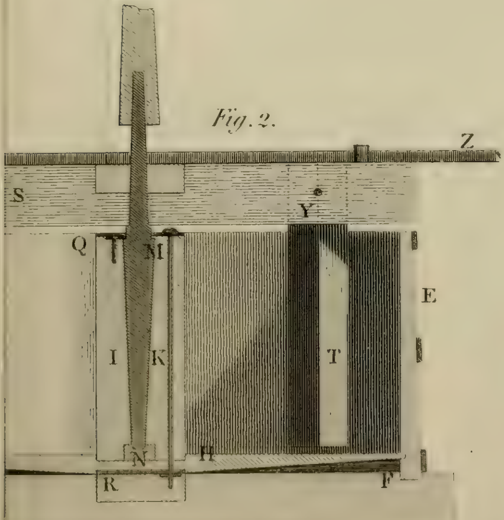

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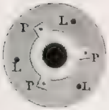

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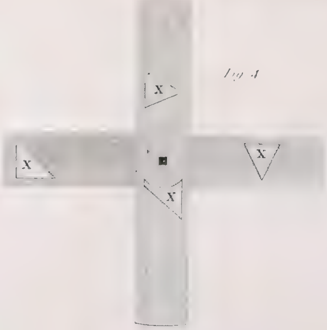

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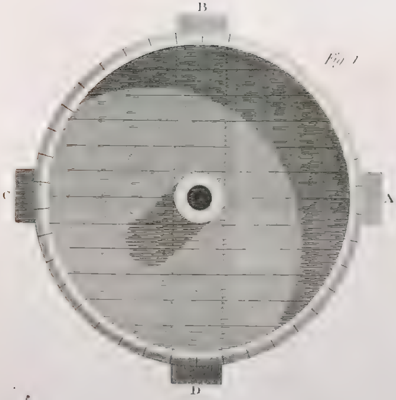

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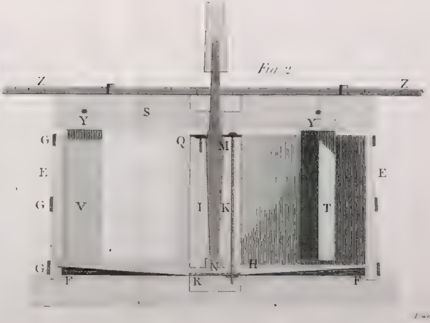

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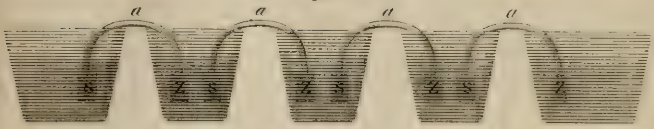

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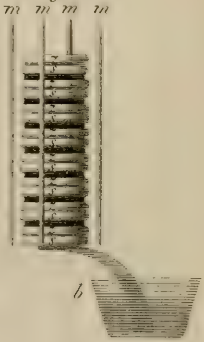

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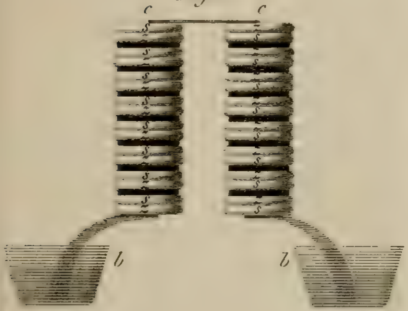

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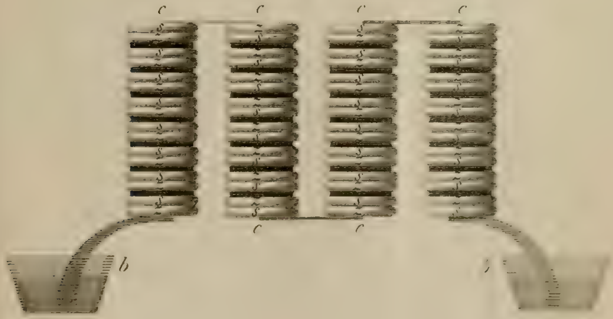

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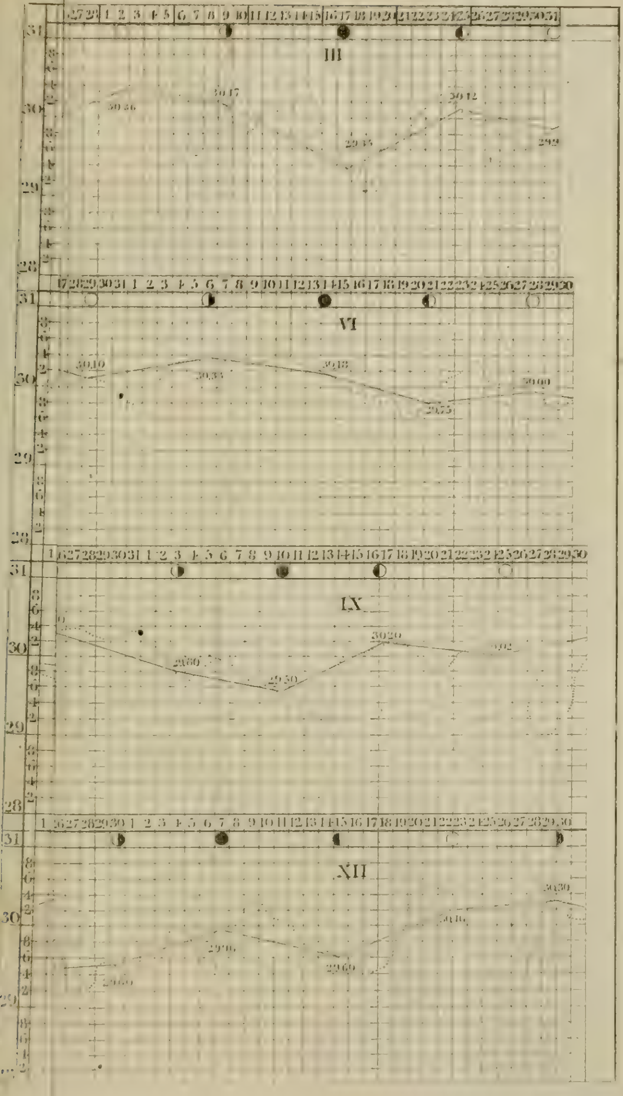

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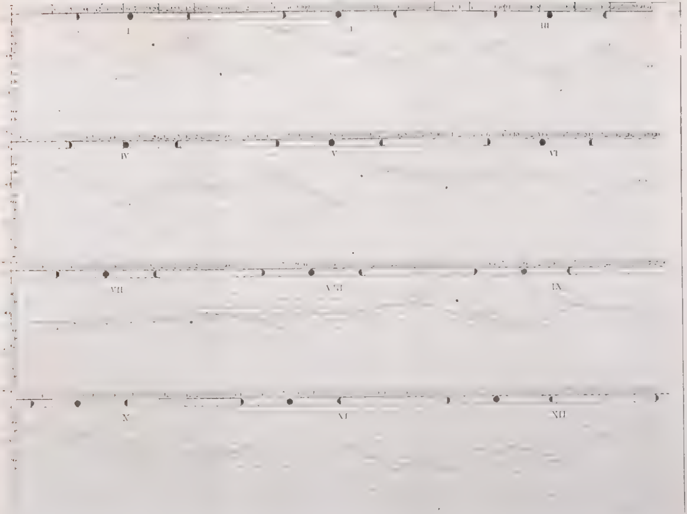

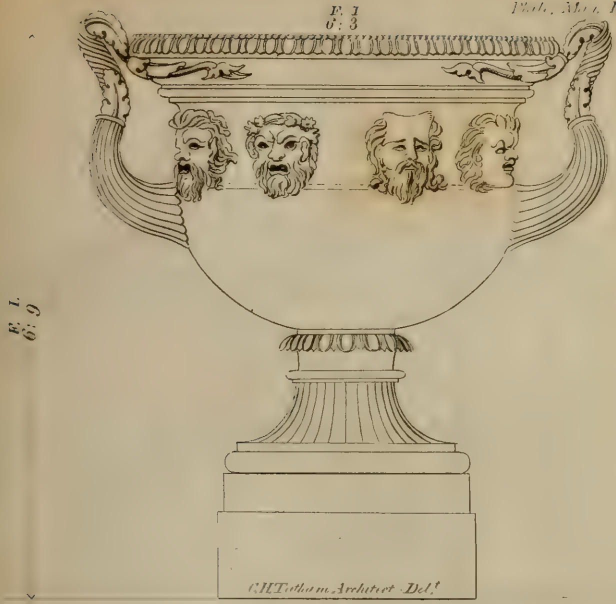
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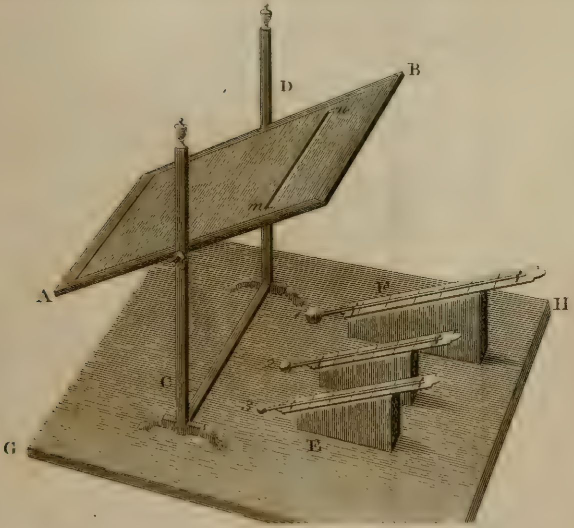
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